

Who Should be Tracked Hovercraft's Heir?

THE Select Committee on Science and Technology has yet to unravel the full mystery associated with the decision taken earlier this year to close down Tracked Hovercraft Limited. Ever since the decision to abandon work at the Earith base of the company was announced, most unfortunately at a time when the select committee had just decided to investigate the future of the project, Mr Airey Neave and his committee have apparently left no stone unturned in their quest for the reasons behind the decision. But, as last week's hearings showed (see page 180) the committee still feels that there is more to be learnt and although its attempts to obtain the minutes of meetings held in the summer and autumn of 1972 might meet with stiff resistance Mr Neave and colleagues will certainly not rest until they are satisfied they know all the facts.

One uncertain aspect of the decision to stop further financial support for the hovertrain is who carried out an appraisal of the company before the final decision was taken. Last week both the chief scientist at the DTI, Dr Ieuan Maddock, and the Director General of Research at the Department of the Environment, Mr D. J. Lyons, did not, in the eyes of the select committee, provide a satisfactory answer to this question. Mr Neave's whimsical aside, on whether it will ever be known who evaluated the future prospects of the company summed up the present situation admirably. But the important aspects of the present inquiry are not solely concentrated around this issue. The deed is done and the decision taken in February to preserve the linear motor work of Tracked Hovercraft Limited through a contract with Hawker Siddeley Dynamics, the precise details of which have still to be determined, needs careful consideration.

According to the government, there is no future for an inter-city transport system based on the tracked hovercraft principle in Britain before 1985, and only limited possibilities between then and the end of the century. It is hard to argue with this although there are some who would say that British Rail should not be basing its future policy solely on the steel wheels on steel rails principle. But it is this conclusion which spelled the end for THL.

But unfortunately the decision did not turn out to be as simple as that and two factors have clouded the issue. First, the decision of the DTI to preserve the work carried out by the company of linear motors and, second, the fact that at the time of the decision to stop supporting THL, the company, in conjunction with Hawker Siddeley, were bidding to build a transit system for the Ontario Government in Toronto.

In the past few weeks both these explanations have been put forward as the rationale for the Department of Trade and Industry's decision to grant £500,000 to Hawker Siddeley to pursue work on linear motors and, unfortunately, both explanations leave something to be desired. Britain, primarily through the efforts of Professor E. R. Laithwaite at Imperial College, is in the forefront of research in linear motors, and it is proper that development work on linear motors should continue. But is Hawker Siddeley the right

company for this? Hawker Siddeley has a redoubtable reputation in the fields of transport and the management of large projects, but palpably no experience of linear motors design and construction. If the object was now to go ahead and build a transport system based on linear motors then there would be little argument that the correct company had been chosen. But the object of granting money to Hawker Siddeley, according to the DTI, was to preserve the research work undertaken by THL on linear motors. A strange decision—if this was the only motive behind it—a company with experience in linear motors would clearly have been a more suitable choice. But it is now abundantly clear that preserving the linear motor research of THL was not the chief reason why Hawker Siddeley was chosen to be the beneficiary of the DTI's generosity. And it was the need for the combined Hawker Siddeley and Tracked Hovercraft Limited bid for the Ontario contract to still have credence after THL ceased operations that led to the DTI's actions.

The DTI is now in an awkward situation. Hawker Siddeley, as luck has it, unfortunately failed to win the Ontario contract—not, as Dr Maddock said last week, because of the demise of THL but because it clearly overbid on the civil engineering side. But the company can still look forward to £0.5 million of government money.

It is now time for the DTI to show that it is really serious about preserving the THL work on linear motors by looking to other companies which have had some previous experience with these motors. It is indeed fortunate that the arrangement with Hawker Siddeley still leaves the DTI open to place contracts elsewhere, and one of the pleasing aspects of last week's hearing was the announcement by Dr Maddock that his department is also talking to GEC, a company with some experience of linear motor work. This will be a more sensible way to proceed now that it is clear why Hawker Siddeley was chosen in the first place. What will indeed be of interest will be the terms of the contract between the Department of Trade and Industry and Hawker Siddeley when they are finally settled.

100 Years Ago



At the special request of Rear-Admiral Sands, the U.S. Congress, at its last session, allowed an appropriation for the purpose of completing and publishing the catalogue of southern stars, observed by Gilliss in 1850-52, and the work is now being put in the hands of computers for publication as soon as possible.

From *Nature*, 8, 94, May 29, 1873



OLD WORLD

Inquiry Continues into Hovertrain's Demise

IF nothing else, last week's hearing by the Select Committee on Science and Technology as part of its inquiry into the decision to abandon the hovertrain project established that Dr Ieuan Maddock, chief scientist at the Department of Trade and Industry is an avid reader of Professor Eric Laithwaite's scientific papers. But Dr Maddock is also interested in what Professor Laithwaite says and he took exception to the statements made to the select committee three weeks ago by Professor Laithwaite about a conversation between the two men at the Royal Institution in February (see *Nature*, 242, 490; 1973).

Professor Laithwaite claimed that Dr Maddock had "clenched his fist and said 'I wish that I had known all this sooner'" when he was shown a demonstration of a new linear motor developed by Professor Laithwaite that produced both lift and propulsion. Dr Maddock, who made it clear in no uncertain terms that he did not like being quoted second hand in public, said that the situation was nothing like Professor Laithwaite claimed. Perhaps, said Dr Maddock, he had clenched his fist, but it was not over being surprised with the demonstration, but probably because Professor Laithwaite had mentioned that he had applied to the Science Research Council for a grant to pursue the work. Dr Maddock being a member of the SRC was surprised that he was not aware of the application.

Dr Maddock and Mr Lyons, Director General of Research at the Department of the Environment, who also gave evidence to the committee, displayed a remarkable degree of coolness towards the proved linear motor developed by Professor Laithwaite and his team at Imperial College in the past few years. It is far from clear, said Dr Maddock, whether in fact building a transport system based on one of these new motors would be more economical than building a system based on the older type of linear motor. And, said Dr Maddock, there were great difficulties associated with the new motor—in particular the repulsion which provides the lift is

known to change into an attraction at high speeds. Such problems had to be solved before the new motor could be considered as the basis for a transport system.

Apart from bringing out this particular disagreement between Dr Maddock and Professor Laithwaite, the select committee also spent some time delving into the reasons why the work of Tracked Hovercraft Limited was halted. It emerged that although the decision was announced in February 1973, a meeting at which Tracked Hovercraft Limited, the NRDC (through which the DTI sponsored THL) and Hawker Siddeley were represented was held in August 1972 with follow up meetings held in October and November. It is the minutes of these meetings which the select committee is now trying to obtain

in order to satisfy themselves of the reasons why support was cut off from THL and also, especially, to discover who, or what group of people, carried out an analysis of the future of THL.

Why was Hawker Siddeley the only industrial company represented at the August meeting, the select committee asked. After a suitable period for thinking and note passing the answer was that the views of industry on the prospects for the work of THL had to be represented and Hawker Siddeley was chosen to represent those views. Before Hawker Siddeley was awarded £500,000 last February to continue the THL work on linear motors, other companies were consulted and were represented at the later meetings, according to Dr Maddock. But the precise way in which Hawker Siddeley is going to spend the

BRITISH ANTARCTIC SURVEY

Icy Problems

THE disappearance over the side of the research vessel *Bransfield* of biological samples from the Antarctic has put back parts of the British Antarctic Survey's work by a year; replacing the lost samples could cost as much as £20,000, according to Sir Vivian Fuchs, the survey's director.

No charges have yet been made in connexion with the loss, and the two members of the ship's crew who were helping police with their inquiries at Shirley police station in Southampton last week have been released, pending the result of forensic studies.

The loss of the samples was discovered after the *Bransfield*, returning from the Antarctic, had left Montevideo on its way home. A survey officer flew out to meet the ship in Las Palmas, but the police were called in when he was unable to clear up the problem.

Apparently the samples were thrown overboard following a dispute over whether beer should be stored in the samples fridge in the *Bransfield*'s scientific laboratory. The fridge contained assorted biological samples which were maintained at -40°C , a temperature considerably lower than that of the domestic refrigerators on board. Andrew Clarke and Michael Richardson, two members of the survey team, objected to the constant opening and closing of the fridge door to get beer out, as, in the hot temperatures of the tropics their

samples were likely to suffer. The crew were finally told to take their beer out of the fridge, and the laboratory was locked. But next day it was found that the laboratory had been broken into and the samples had gone.

The samples included assorted crustacea and algae, including some examples of *Boeckella sylvestri*, fresh moss, mineral ash, marine benthos and extracted lipids, as well as whole and dissected birds and fish, and blood and tissue samples taken from expedition members at the Signy Island base which were to be used in the physiological studies of the effects on man of living at extremely low temperatures for extended periods. Samples of *Notothenia neglecta*, the "Antarctic cod", and *Chaenocephalus aceratus*, one of the Antarctic "ice fish" which live in the very cold waters close to the ice shelf and which are "bloodless", having no haemoglobin, the oxygen being transported round the body in their blood plasma, were also lost.

The lost samples represent more than a year's work for the two biologists, and affect the work of four other scientists still at Signy Island. Clarke and Richardson, both of whom had been in the Antarctic for two and a half years, were there to write reports in the British Antarctic Survey's scientific series, and would probably have used the results of their work to apply for PhD degrees at a British university once the full studies on the specimens had been completed at Monks Wood research station. As it is, much of the work will have to be repeated.

Nature 1972 Index

THE combined index to *Nature*, *Nature New Biology* and *Nature Physical Science* for 1972 is now available. A subscription form is to be found on page iv.

money from the DTI has yet to be determined—according to Dr Maddock the money was awarded on the basis of agreement and the details are being worked out at present.

But was Hawker Siddeley awarded this money solely to develop research on linear motors? It seems not, for it became clear at the select committee meeting that the DTI was concerned at the beginning of the year that the demise of THL could jeopardise the combined THL and Hawker Siddeley bid to build a transport system in Toronto, Canada. So to a certain extent the money was awarded to maintain the credibility of the bid. Since then, of course the Toronto contract has been awarded to Krauss Maffei a German company.

But the contract with Hawker Siddeley is not exclusive and the DTI is at present discussing with GEC the possibilities of it doing some work on linear motors.

MARITIME SATELLITE

GTS May Not Do

EUROPE is unlikely to be taking a share in Britain's Geostationary Technology Satellite. At a meeting of the European Space Research Organization's joint programmes and policy committee last week, a comparison between GTS and an adaption of ESRO's own Orbital Test Satellite as possible maritime satellites was presented which heavily favoured the adoption of the ESRO satellite rather than GTS.

No decision has yet been taken in Europe to develop a maritime satellite to improve ship to shore radio contact, but ESRO has been undertaking studies for some time, and a performance specification for a maritime satellite has been drawn up, which has been largely approved by the Inter-governmental Maritime Consultative Organization (IMCO).

Britain offered GTS as a possible base for a test maritime satellite at last December's European Space Conference in Brussels at which the decision to attempt to rationalize Europe's space programme was taken. But ESRO's assessment of it argues that its facilities would not be adequate as a pre-operational satellite when compared to the facilities that ESRO's adapted Orbital Test Satellite could offer. GTS, being a completely new project, would also cost more, ESRO says.

GTS at present is still in the project definition stage. Conceived in mid-1971, its aim was to demonstrate high quality communications links and operative experience of various communications services, as well as to allow investigation of the propagation of radio frequency waves and of new British spacecraft systems.

The offer of it as a pre-operational

maritime satellite falls down chiefly because it is designed as an experimental technology satellite which will provide some operational experience, rather than as an operational satellite which also provides some experimental facilities, which is the concept behind the ESRO satellite.

It is not entirely surprising that ESRO's proposal meets the performance specification rather more closely than GTS. When the GTS proposal was submitted at the end of March the specification had not been drawn up, and ESRO's secretariat drafted the specification at the same time as the specification for ESRO's own proposal was worked out.

But given the performance specification that has emerged from ESRO's consultation with interested parties and with IMCO, GTS plainly fails to meet it.

The performance specification calls for a satellite with 17.5° "global coverage" antenna beams to service an area stretching from Panama to the Persian Gulf in order to cover the principal tanker routes and 50 per cent of the world's chief shipping movements. These antenna should provide satellite to ship and satellite to shore links. GTS, however, can only provide a 7° antenna beam designed to cover the North Atlantic in the satellite to ship band, and a 1° beam in the satellite to shore band—a beam that could only cover a small part of Europe. GTS also only provides eight channels, whereas the specification calls for sixteen, although GTS does provide experimental facilities to use time division multiple access (TDMA), which IMCO has urged should be studied, whereas ESRO's proposals can provide only frequency division multiple access (FDMA).

Nonetheless ESRO's proposal meets the MARSAT specification and GTS does not.

But whether the British proposal will in fact find favour when the choice between the two systems is raised at the ESRO council meeting planned for June 1, will depend on several factors. Partly it will depend on whether the council wants an experimental or an operational capability (and the latter seems more likely) and partly on the results of the inevitable bargaining that will take place on other European space ventures such as Spacelab, L3S, (the new launcher proposed by France) and the need to spread Europe's money as far as possible. One benefit from Europe's point of view in the British proposal is that Britain will shoulder 75% of the cost. If ESRO's version of a maritime satellite is built, Britain would not have to provide more than the Gross National Product share (23%) that she normally puts into ESRO programmes.

SOVIET SCIENCE

Future Difficulties?

from our Soviet Correspondent

THE forthcoming meeting of the joint Soviet-United States team to discuss the Soyuz-Apollo project, which is scheduled for 1975, will take place under what are undoubtedly unfavourable conditions as far as the Soviet side is concerned. Although no official statements have appeared in the Soviet press two major setbacks occurred to the Soviet space programme last month.

Over the years, the Soviet space programme has had more than its fair share of failures. Some, such as the abortive launch on November 26, 1965, of a Venus probe, were "disguised" under the cover-all of the Kosmos programme. Failures on the launch-pad apparently go unreported, although reconnaissance satellites from the United States have observed four "disappearances" from the launch-pad of large rockets which have never gone into orbit. The most recent of these was at the end of November 1972. Even the failure last month of a major lunar probe, carrying what should have been Lunokhod-3, has passed without official comment. Only when loss of life is involved does Baikonur admit failure.

The failure of the Salyut-2 station, however, could not pass unremarked. A successor to the Salyut-1 of 1971, its launch was reported in *Pravda* on April 4 accompanied by an article on the Soyuz-Apollo project which pointed out that the docking fault which led to the deaths of the three cosmonauts who visited the Salyut-1 station had been overcome, and Salyut-2 would in due course be visited by a crew aboard a Soyuz. No such visit took place, however, and on April 12, the traditional Soviet "Cosmonauts' Day" (the anniversary of Gagarin's flight), it was announced that Salyut-2 was carrying out its assigned mission—the testing under remote control of on-board systems. Observers reported, however, that one of the solar panels of Salyut-2 had become detached, so that clearly the station was no longer in working order for a manned visit. Later TASS announced that Salyut-2 had "completed its mission" satisfactorily.

When the Soyuz-Apollo mission was first suggested it was considered that the chief problem would be to decide on whether to use air at reduced pressure or oxygen as the atmosphere. The language barrier was also considered to present difficulties (see *Nature*, 239, 362; 1972). According to Academician Petrov (*Pravda*, August 2, 1972) work on the mechanics of the link-up is already in progress. But doubtless the problems that Skylab has run into are occupying the Americans more at present.

NEW WORLD

AEC Promotes Safety Research

by our Washington Correspondent

AMID rumours of a palace revolt and Congressional displeasure, the Atomic Energy Commission is going ahead with a reorganization plan designed to give more prominence to research on reactor safety. The plan represents an abrupt *volte face* on the part of most of the Atomic Energy Commissioners and it may lead to the resignation of Milton Shaw, the powerful head of the AEC's Division of Reactor Development and Technology. But, on the other hand, it may help to head off criticisms that reactor safety has suffered from conflicts of interest within the agency, and it seems to represent a victory for the AEC's new chairman, Dr Dixie Lee Ray.

The essence of the reorganization plan is that safety research on water-cooled reactors is being taken out of Shaw's division and placed in a division of its own. Dr Herbert J. C. Kouts, a nuclear physicist at present employed at Brookhaven and a former chairman of the Advisory Committee on Reactor Safeguards, has been appointed director of the new division and will report directly to the AEC's General Manager. According to Dr Ray, the changes will "provide for greater emphasis and effectiveness in (the AEC's) safety research programmes", and they will "help to speed resolution of the still-unanswered questions in this rapidly developing technology".

Such a relatively minor bureaucratic reshuffle may seem an insignificant matter, but it represents a rather fundamental change of view within the commission, and it comes at a time when public concern over reactor safety is at its height. It seeks to redress a constant criticism of the AEC, namely, that it suffers from a conflict of interest that arises from its dual responsibility for promoting and regulating nuclear power. As far as safety is concerned, the agency's critics have suggested that since responsibility for safety research has been vested in the division which is also responsible for reactor development, it has not received the attention it deserves. The reorganization plan is thus an attempt to remove the appearance of conflict of interest and to elevate safety research to a higher position in the AEC's bureaucratic hierarchy.

But the reshuffle has not pleased everybody. Among those who are not enamoured with it are Milton Shaw, who has lost some of his responsibilities, Commissioner James Ramey and a few

powerful members of the Joint Committee on Atomic Energy, the AEC's Congressional watchdog. They are concerned that since safety systems are such an integral part of reactor design, to divorce safety research from reactor development could be counterproductive.

But those who favoured splitting off safety research into a separate division—and Dr Ray was among them—argued that as far as water cooled reactors are concerned, development is essentially complete, and so there is no need to integrate safety research with development. The same cannot be said for the fast breeder reactors, however, and so the final reorganization plan keeps research on breeder reactor safety under Mr Shaw's wing.

The fact that a majority of the five-member commission eventually agreed to the reorganization plan represents a complete change of heart during the

past year, for in April 1972 there was a move to shift responsibility for safety research to the AEC's Regulatory Division, but the commission was unanimously opposed to the idea. The Joint Committee on Atomic Energy in fact then held a day's closed hearings to discuss the matter with commission members, after which James Schlesinger, who was then AEC chairman, wrote to the committee to say that "removing the safety research function from the Division of Reactor Development and Technology would have detrimental effects upon the overall safety of reactors". Last autumn, however, Schlesinger proposed his own scheme to set up an office of safety analysis reporting directly to the commission, which would liaise between Shaw's division and the regulators. Commissioner Ramey said last week in a telephone interview that he still favours that scheme rather than the reorganization

ENVIRONMENT

Out with a Bang?

by our Washington Correspondent

THE latest explosion in the AEC's Plowshare series shook a few buildings and raised some dust in Colorado last week, but it seems from preliminary reports that the blast triggered little damage and met with general apathy among local residents. Whether the test successfully achieved its objective of fracturing layers of rock deep under the surface to release trapped natural gas will not be known for several weeks, when a shaft will be redrilled to the cavern made by the explosion and the quantity of gas trapped there will be measured. Even if the test is an unqualified success, however, there are serious doubts about whether nuclear explosives will ever be used to exploit the vast deposits of natural gas trapped in tight formations beneath the Rocky Mountains (see *Nature*, 241, 494; 1973).

The test, called Rio Blanco, ran into some opposition before it took place—environmentalists tried unsuccessfully to get it stopped by the courts, and Senator Floyd K. Haskell, the new Senator from Colorado, held a day of hearings into the project, but if the AEC and CER Geonuclear Corporation of Las Vegas, its industrial partner in the exercise, propose to take the project any further, they are likely to have to fight tooth and nail in the courts and to overcome some opposi-

tion in Congress and in other government agencies. The chief problem will be that, according to an estimate from the Lawrence Livermore Laboratory, it will take 370 blasts each year until the end of the century to exploit all the gas deposits underneath the Rockies—local residents may be willing to put up with one test shot, but not with several thousand.

The chief fears which have been voiced about the project are that the radioactivity from the blasts will seep into groundwaters and contaminate the Colorado River, but the AEC vigorously denies the possibility of that happening. The question has also been raised of the dangers that may arise from radioactivity in the released gas itself, but Dr Gerald W. Johnson, head of the Plowshare programme, told the Haskell hearings that a consumer would receive an annual dose of radioactivity from the gas about a hundred times smaller than that from a colour television set.

Rio Blanco itself was designed to test whether firing three explosives simultaneously would release more gas than a single shot—the test involved three 300-kiloton devices in a vertical line—and to test a low-yield explosive. The results will enable AEC officials to balance the possible risks associated with development of the gas field with the benefits of being able to double proven reserves of natural gas in the United States. There is a chance, however, that it may have been Plowshare's last blast.

now being put into effect because it would have left the Division of Reactor Development and Technology intact.

As for the Joint Committee, it met in closed session with Dr Ray last week to talk over the plan, and although no formal vote was taken, a majority of those present favoured the reshuffle. A few were not happy about it, however, and one of their chief concerns was that it may precipitate the resignation of Mr Shaw. A forceful administrator, Shaw is in charge of the development of the liquid metal fast breeder reactor, on which the Administration has pinned its hopes for the next generation of reactors, and some committee members were anxious that his possible resignation may hamper development of the breeder. Sources in the AEC and in Congress confirmed last week that Shaw is upset by the reorganization and at least toying with the idea of quitting.

What made the commissioners change their minds during the past year? A number of factors played a part, but the most influential was probably the public hearings last year on the emergency core cooling device (ECCS) which is supposed to flood the reactor core with water to prevent a melt down if the main cooling water were lost through a sudden pipe rupture. The hearings, which took place over more than a hundred days and which produced some 20,000 pages of oral testimony, were held because of doubts about the ECCS after test failures in 1971.

The public hearings brought to the surface a number of disturbing allegations that the results of safety research which casts doubt on the safety of reactors have been suppressed by AEC officials. It also became evident during the hearings that a number of scientists working for the AEC believe that the commission's standards are not strict enough. Whatever the merit of such allegations, the AEC moved a little towards the position of its critics last October when L. Manning Muntzing, the Director of Regulation, announced stricter operating criteria which may reduce the power output of some reactors, until doubts about their safety are cleared up.

Another factor in the commissioners' change of mind is that the Joint Committee will soon be holding its own hearings on nuclear safety. The hearings have been promised for the past six months, but the committee has been waiting for a comprehensive report on the matter from the AEC. The report is now reported to be in the final stages of revision, and should be delivered soon. Those hearings are likely to capture considerable public attention, and by moving now to head off charges of conflict of interest in safety research, the AEC may take some of the wind out of its critics' sails.

ENVIRONMENT

Voice in the Wilderness

by our Washington Correspondent

RUSSELL Train, Chairman of the Council on Environmental Quality, earlier this month lashed out at some segments of the oil industry and others who have made "the environment the whipping boy for our energy problems". Speaking at the American Power Conference in Chicago, Train criticized automobile and oil companies which recently conducted a massive advertising campaign charging that devices to control exhaust emissions from automobiles will increase fuel consumption. Such advertisements, Train pointed out, ignore the fact that comparable fuel losses come from the use of automatic transmissions and air conditioners, and that a vehicle weighing 5,000 pounds consumes twice as much fuel as one weighing 2,500 pounds—ten times the fuel loss from emissions controls. "Half truths are not going to help meet our energy needs," he said.

Train also sought to dismantle another common myth—that environmentalists have been responsible for delaying nuclear power plants. He said that of 75 plants now in various stages of construction, only about nine have been delayed by environmental challenges.

Moreover, in January of this year, he said, the Atomic Energy Commission had under review the operating licences for 35 nuclear power plants, and "with the possible exception of one or two units, the commission anticipates no delays solely on the basis of environmental considerations in the issuance of operating licences for these reactors". And environmentalists can hardly be blamed—as they often are—for the lack of refining capacity in the oil industry, Train suggested, because until the second half of last year, refineries in the United States were working at less than 85 per cent of their total capacity.

Train's remarks stand out like a sore thumb against statements of several other members of the Administration. Recently, for example, Earl Butz, Secretary of Agriculture, suggested that environmentalists should be the first to have their power shut off when energy supplies run low.

SEISMOLOGY

From NOAA to USGS

by our Washington Correspondent

THE Office of Management and Budget has finally given its approval for some of the seismology programmes and facilities of the National Oceanic and

Atmospheric Administration (NOAA) to be transferred to the US Geological Survey. The move will probably take place in two phases, the first of which has been agreed and will take place on May 27, while the second is still under negotiation between officials of NOAA, OMB and the National Science Foundation. NOAA was forced to abandon much of its earthquake research because of cutbacks in its funds as part of President Nixon's drive to hold down federal expenditure (see *Nature*, **241**, 362; 1973).

The facilities involved in next week's transfer are those engaged in engineering seismology, earthquake prediction and earthquake hazards assessment, chief of which is the Earthquake Mechanisms Laboratory (EML) in San Francisco.

A spokesman for the laboratory said last week that the laboratory staff expects to carry on operations relatively unaffected by the transfer, and few harsh words have so far been spoken about the moves.

Phase two of the possible transfer has not, however, been agreed to by OMB, and it may raise some more opposition.

If it goes ahead, NOAA would lose responsibility for the World Wide Standard Seismograph Network (WWSSN), the National Earthquake Information Center in Boulder, Colorado, and all the other seismic observatories except those which are needed for the tsunami warning system. At present, NOAA has sufficient funds in its 1974 budget to fund the facilities involved in phase two of the transfer plan.

One concern is that seismic data should still be readily available to geophysicists under any system that is worked out.

Already tentative plans call for the National Earthquake Information Center to continue to provide raw data on the preliminary location of epicentres of seismic events, while NOAA's Environmental Data Centers (which it will retain) will continue to provide more complete data. In other words, if the full plan goes ahead, the same facilities will continue to provide geophysical data as at present, but one will be under control of the USGS, while the other will remain with the National Oceanic and Atmospheric Administration.

One advantage of the scheme will be that planning and administration of the federal government's earthquake research will be under a single roof. In the past there has been considerable criticism of duplication of efforts and lack of cooperation between NOAA and USGS, and such problems should no longer arise once the new scheme is in operation.

NEWS AND VIEWS

Looking at the Hydrogen Economy

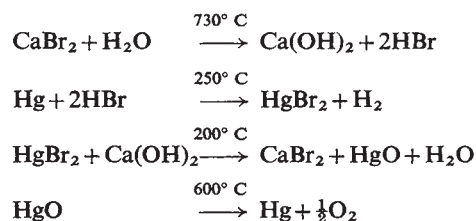
THE sudden recognition that an energy crisis is facing the world has loosed upon that same long-suffering world an avalanche of articles minatory, pontifical or plain gloating: heaven knows how much energy has been used in the production and absorption of these writings. Most of the prophets of doom are concerned only with present fuels and present modes of extraction and use, and very properly—given their premises—come to deeply pessimistic conclusions. A broader approach leads to less apocalyptic conclusions: thus Surrey and Bromley, in their contribution (*Futures*, 5, 90; 1973) to the University of Sussex's analyses of the Club of Rome's *The Limits to Growth*, after concluding flatly that "there is simply no way of accurately assessing total world energy resources", rehearse the reasons for confidence that technological innovation can keep the crisis under control by developing new energy sources, new ways of using present ones more effectively, and ways of damping down the more profligate forms of energy waste.

Another voice of qualified optimism is that of Sir Alan Cottrell, the British Government's Chief Scientific Adviser. Last October he presented a survey lecture on materials and energy to members of the two principal American metallurgical societies, and this has now been published (*Metall. Trans.*, 4, 405; 1973). His theme is the central part that constructional materials must play in the more efficient extraction and exploitation of fuels and in the more economical use of the resultant power: in the process, he covers with his usual clarity and verbal economy a very wide range of devices for extracting energy from fuels, and in particular focuses attention on some aspects which are usually neglected, especially techniques of storing energy. His article, most of which is accessible to non-scientists, should certainly be read by industrialists and publicists concerned with the future of energy.

In passing, Sir Alan repeatedly refers to the suggestions which have been made for the widespread use of hydrogen as a vehicle for converting heat into an easily transportable and storable form of energy. He quotes the electrochemist Bockris (*Science*, 176, 1323; 1972) as one recent proponent of this notion: it has also recently been analysed in *Scientific American*. Water would be electrolysed at nuclear power stations, presumably at night at periods of low electricity demand, and the hydrogen distributed through the natural gas pipe network, refrigerated and stored in liquid form in giant Dewar vessels. One way of using it, as Cottrell points out, is for direct conversion into electricity at high efficiency by means of Bacon fuel-cells. A supplement to this proposal has recently been studied in the United States: liquid hydrogen proves to be an efficient and entirely pollution-free fuel for existing motor car engines, and a serious analyses of the advantages and problems of using it in this way is now in progress.

As outlined, the proposal for a "hydrogen economy" involves hydrogen merely as a convenient intermediary, plus a means of absorbing electricity at periods of low demand and thus improving return on capital. But hydrogen could in principle be used to better effect. Cottrell points out the low overall efficiency, even now, of heat

engines and their attached electric generators, and the benefits which will attend really effective large-scale direct conversion of heat into electricity or other forms of expressed or stored energy. The production of hydrogen by electrolysis is not efficient: apart from the inescapable thermodynamic limitations on the conversion of "nuclear heat" into electricity, large industrial electrolyzers, according to Marchetti (*Laser*, No. 34, 11; 1973), are only about 50% efficient and very capital intensive. Marchetti, who is in charge of materials research at the Euratom Laboratories at Ispra, Italy, makes this point in the course of a detailed account of the chemical hydrogen extraction process on which a team at Ispra has been working for some time. The Euratom method involves a cycle of several reactions. Several variants have been examined; the mark I cycle is as follows:



The sum of these is: $\text{H}_2\text{O} \rightarrow \text{H}_2 + \frac{1}{2}\text{O}_2$.

According to Marchetti, the thermodynamic efficiency of this cycle is about 85%, and the practical efficiency overall is at present about 55%, and still improving. Materials seem to pose no serious problems and recovery of the catalysts is straightforward. He goes on to a detailed analysis of the economics, in terms of capital costs and running costs assessed separately, of the generation and transport of hydrogen. The further implications of his proposals are to be discussed in the next issue of *Laser* (a scientific magazine published privately from 26 Selwood Road, Addiscombe, Croydon).

From information publicly available it would seem that at present all the running in this field is being made by American industry and by Euratom. It is to be hoped that the powers that be in Britain are at least maintaining a close watching brief at this stage.

The growing interest in the use of liquid hydrogen as a transportable, highly concentrated form of fuel can be regarded as a form of technology transfer. The American space programme has turned liquid hydrogen from an alarming speciality (used largely for nuclear bubble chamber research) into a familiar and not particularly dangerous commodity. But it is one thing for a large government agency to handle a material which was formerly thought unacceptably hazardous and quite another for the man in the street—or the roadside garage man—to do likewise. The precedents are not encouraging: liquid sodium is another material in the same category as liquid hydrogen. It has become familiar within specialized engineering circles because of its essential use as a coolant for certain types of nuclear reactor, and has been handled with complete safety for years past. Nevertheless, a brave attempt by a Union Carbide subsidiary to manufacture plastic-

sheathed sodium cables for the transport of electricity has failed and the factory has had to be closed.

From our Materials Science Correspondent

Synaptic Ghosts

THE arresting phrase "ghosts of childhood" was used by Merrill and Wall (*J. Physiol.*, **226**, 825; 1972) when they were discussing the presence of ineffective synaptic connexions in the spinal cord of adult cats. It had previously been widely assumed that the existence of a morphological synapse between two neurones indicated a functional connexion, either excitatory or inhibitory, between these two cells. Merrill and Wall, however, described synapses that were effective only if activated artificially, by electrical stimulation, and were not driven by natural stimuli. They suggested that these synapses may have been naturally active early in development, only subsequently losing their effectiveness.

Mark and his colleagues (*Brain Res.*, **19**, 41, 53; 1971; and **46**, 131, 149; 1972) had earlier described a related phenomenon after regeneration of the oculomotor and trochlear nerves in fish. The normal nerve supplying an eye muscle was cut and that muscle forced to receive innervation from a foreign nerve. Functional synapses were established between the foreign nerve and muscle. When the normal nerve regenerated into the muscle, not only did it establish functional synapses, but its arrival also repressed the activity in the previously functional synapses formed by the foreign nerve. This repression occurred very rapidly and in the absence of observable morphological changes in the repressed synapses.

On page 201 of this issue of *Nature*, Cass, Sutton and Mark report on the finding once again of ineffective synapses, in this case in the salamander limb. Electrical stimulation of a segmental nerve supplying the limb yielded muscle contraction and functional potentials within a well defined muscular territory. Section of that segmental nerve, however, produced degenerating synapses which extended beyond the functionally defined territory of that nerve. Conversely, within the territory were found undegenerated synapses deriving, presumably, from other segmental nerves, although electrical stimulation of these latter nerves did not produce synaptic activity within the territory. This non-functional innervation occupied a band about 0.5 mm wide on either side of the territorial boundary. These ineffective synapses in the salamander differ from those described in the cat in that they are unresponsive even to electrical stimulation.

The presence of such synapses raises two questions. What do they represent, and what is responsible for the repression of their activity? Cass *et al.* suggest that chemospecific developmental forces direct nerve fibres to approximately their natural muscle territories but this process is refined by a competition between terminals for the control of muscles. The authors are a little vague as to the nature of this competition implying, in one instance, that it may be based on selective chemospecific recognition between nerve and muscle cells and, in another, suggesting that relative amounts of impulse traffic in competing terminals may be the critical factor. The end result of the competitive process is not the destruction of the defeated synapses, but their functional repression, leaving

a fringe of functionless synapses bracketing the territorial boundaries. The mechanism of the repression is not known, but the experimental situation indicates that it occurs at the neuromuscular synapse. The repression is not, however, an irreversible change occurring once and for all during development, but rather seems to be a dynamic on-going process. Function can reappear at these repressed synaptic sites.

The spinal nerve supplying a muscle territory was cut and the muscle was paralysed. Within 2 to 3 days, however, stimulation of adjacent nerve roots led to activity in the boundary fringe region. Cass *et al.* suggest that this early appearance of activity reflects the emergence of function in pre-existing but functionless nerve terminals. One to two weeks later, activity appeared in the remainder of the denervated muscle and this was attributed to collateral sprouting from fibres of adjacent nerve roots. Later, the original sectioned nerve regenerated back into the muscle, re-established functional synapses and suppressed all the foreign innervation. The suppressed synapses do not apparently disappear because cutting the normal nerve a second time resulted in the rapid derepression of foreign innervation; in this case activity appeared within 2 to 3 days throughout the denervated muscle instead of just at the boundary fringe. This conclusion is plausible but should be confirmed by the demonstration that after regeneration of the normal nerve, section of an adjacent root produces degenerating synapses throughout the muscular territory of the normal nerve.

The concept that during development synaptic relationships are established more widely than is apparent from the adult pattern is not new—it was one of the principal thrusts of Cajal's speculation on neurogenesis. What is new is the notion that these excess synapses, or a proportion of them, survive in a morphologically normal, albeit functionally ineffective, form. The substantiation of this view would mean that one could no longer accept that the anatomical demonstration of a synaptic connexion is necessarily equivalent to the demonstration of a functionally effective pathway. The suggestion by both Merrill and Wall and Cass *et al.* that functional activity in competing pathways may determine the pattern of effective synapses seen in the adult animal deserves further investigation.

From a Correspondent

Foetal Antigens and Cancer

ENTHUSIASM for the notion that the growth of malignant cells is regulated by an immunological mechanism has, as Prehn has pointed out, waxed and waned over the years. Thomas initially put forward the idea that there might exist an immunological surveillance mechanism operative against potentially malignant cell populations and Burnet has popularized this theory. Experimental validation for it has been strangely slow to accumulate although, in recent years, the relatively high incidence of malignancies in certain groups of chronically immunosuppressed patients has been cited as evidence for the hypothesis. In spite of these uncertainties many attempts are presently being made to apply immunotherapeutic techniques in the treatment of various kinds of cancer, and the concept that malignant cell populations are antigenically different from the normal cells

from which they derive is widely held to be useful.

One view is that cancer cells sometimes or always express antigens which are normally only to be found or are only found in large amounts in foetal tissues. Seen thus, malignancy is a form of dedifferentiation. The carcino-embryonic antigens and α -foetoproteins which are commonly found associated with certain kinds of cancer and which normally are characteristic of foetal tissues exemplify this point. The notion is attractive and is accumulating evidence. On page 225 of this issue of *Nature*, Castro and his colleagues report an experimental approach to the problem which might eventually show how an understanding of foetal antigens can be used in cancer therapy and perhaps even prophylaxis.

A mixed bag of foetal tissues was inserted under the kidney capsule of syngeneic or allogeneic recipient mice which were either normal or immunologically incompetent (by prior thymectomy, irradiation and bone-marrow injection). In normal mice neither syngeneic nor allogeneic foetal tissues grew well though syngeneic adult cells did. In the mice which were immunologically deprived foetal tissues of both kinds grew well and manifested differentiated cells of many sorts. Castro and his colleagues suggest that the failure of syngeneic foetal tissues to grow well in normal mice may indicate that there is a response against foetal antigens. Acting on this suggestion they went on to induce or transplant tumours in mice which had "rejected" syngeneic foetal tissue. In neither instance was a suppression of the malignancy observed; quite the contrary, the induction time was shortened and the growth of the transplanted tumour was enhanced. It might have been better had tumours which are known to express particular foetal antigens been used rather than chemically-induced tumours of ill-defined antigenicity. Nevertheless these studies do point the way for further and more specific experimentation.

From a Correspondent

CHEMOTAXIS

Nematode Orientation

from a Correspondent

NEMATODES are largely studied as parasites in man, animals and plants and few critical data on their behaviour and



Tracks of three wild type adult *C. elegans* responding to gradients of NH_4Cl in a thin layer of agarose. The worms were placed on the plate at the points marked by dots and were allowed to make tracks for 15 min. The gradient ranged from 50 mM in the centre of the plate to 0.05 at the edge, and it changed little during the time of tracking (from Ward, *Proc. US Nat. Acad. Sci.*, **70**, 819; 1973).

sensory physiology are available. *Caenorhabditis elegans*, and a few other nematodes, are, however, becoming the focus of considerable attention as basic biological models. *C. elegans*, which is about 1 mm long, can be maintained in the laboratory on bacteria or in axenic culture. As with other nematodes, many tissues, including the nervous system, have constant numbers of cells, and there are less than 300 neurones in *C. elegans*. It has a generation time of a few days, is a protandrous hermaphrodite, can be mutated in ethyl-methane sulphonate and the mutants can be stored in nitrogen.

Ward (*Proc. US Nat. Acad. Sci.*, **70**, 817; 1973) believes that the study of behavioural mutants can lead to a correlation of behavioural alterations with underlying changes in anatomy, physiology and biochemistry. This will identify the neural circuitry mediating the behaviour and may reveal molecular mechanisms by which nerves operate. The ultimate correlation may be between single gene defects and specific patterns of behaviour.

Ward used the wild type and selected mutants of *C. elegans* to study its chemotactic behaviour and set out to identify the attractants, the location of receptors, and the mechanism of orientation. Chemical gradients were established in 'Sephadex' or agarose and the orientation to and accumulation within high concentrations of attractants did not differ between larval stages and adults. *C. elegans* moved towards cyclic AMP and cyclic GMP at initial concentrations of 0.2 to 3.2 mM, but were not attracted by 3'- CH_2 AMP, N^6O^2 -dibutyryl-cyclic AMP, 3' AMP and 5' AMP. Certain, but not all, anions and cations were

attractive, as were OH^- and some amino acids, but sugars were not. When cyclic AMP at 2.5 mM was distributed uniformly in 'Sephadex' before the establishment of a cyclic GMP gradient, normal orientation to cyclic GMP was eliminated. This suggests that both molecules act on the same receptor site. The presence of cyclic AMP did not affect orientation to Cl^- , and Cl^- and Na^+ did not compete. At concentrations of 0.3 M and above, the salts were repellent.

When moving over 1.5% agarose, *C. elegans* inscribes tracks and detailed analyses of its orientation are possible (see figure). Ward found that wild type individuals swam in relatively straight lines up to the gradients, but outside gradients or in areas of uniformly high concentrations, they turned, spiralled and frequently reversed. A slow mutant with degenerate muscles oriented equally as well as the wild type, but took eight times as long, demonstrating that orientation is independent of the forward velocity. Ward concludes that orientation is determined by the lateral movement of the head, and is therefore by clinotaxis. Results of experiments on other mutants with cuticular "blisters" support the notion that the amphids are chemoreceptors, an assumption which is likely to lack electrophysiological support for some time. The sensory receptors on the tail, the phasmids, seem not to be involved in the orientation response.

POLYPEPTIDES

The Missing Magnitudes

from our Molecular Biology Correspondent

PERHAPS the best justification that nowadays remains for the study of synthetic polypeptides as a twig, if not a branch, of molecular biology is that they allow theories of the kinetics of conformational transitions to be put to the test, and that the analysis of the kinetics of rapid processes in general has an undoubted relevance to a variety of interesting biochemical systems. The theory of the order-disorder transition of polymers in solution has reached a state of considerable refinement, and the indications are that it can account rather well for almost all the available experimental facts. As regards the archetypal case, that of the α -helix-random coil transition in synthetic homopolymers, quantitative data are sparse, however, since relaxation times in the micro-second region are involved, which are not accessible by the temperature-jump or related techniques. With the use of the Ising lattice formulation, Schwarz calculated the kinetic characteristics of such transitions taking account of the existence of nucleation and propagation processes in the formation of ordered

structure, and arrived at the expectation of a maximum relaxation time at the transition midpoint—which, the indications are, is nevertheless still in the sub-microsecond range.

The general accord on the characteristics of the transition was breached only by the unexpected observation that proton magnetic resonance was able in certain cases to resolve contributions from α -carbon proteins in the α -helical and the randomly coiled states. Now this would imply slow interconversion between these conformations relative to the nuclear relaxation time scale, in other words a transition rate which is a round half-dozen orders of magnitude slower than it should be. Various explanations were offered for this phenomenon, of which only two survived closer scrutiny. One was that the polypeptide samples are all polydisperse, and because the longest chains form the most stable helices, and exhibit the sharpest transitions, one is simply resolving the signals from different size populations. The other suggestion was that the helix-nucleation step is slow in the polymer-solvent combinations in question, and the distribution of conformations at equilibrium is in fact very different from that calculated from the Ising lattice model.

This proposition has now been re-examined by Miller (*Macromolecules*, **6**, 100; 1973), who has calculated conformational lifetimes for more elaborate models. In particular he has examined the effect of the assumption, made for ease of computation, that the probability of finding more than one run of helix within a single chain is low. This he shows leads to an overestimate of the helix lifetime. The values for the lifetimes also depend on the balance of the thermodynamic parameters for helix formation and the chain length. By choosing a combination of values for the parameters in the model, Miller is able to simulate the observed NMR spectra. That the splitting of helix and coil contributions has been observed only in non-aqueous solvent systems can be easily enough rationalized on the basis of different values for the helix initiation parameter in the two cases.

It only partly detracts from the interest of this analysis that the NMR results which it seeks to explain are evidently in fact no more than a consequence of polydispersity. In the interim Nagayama and Wada (*Chem. Phys. Lett.*, **16**, 50; 1972) have examined very narrow cuts of polybenzylglutamate (with a weight to number average molecular weight ratio of 1.04), and shown that there is no α -carbon proton splitting in the helix-coil transition region, thereby, one may hope, laying the ghost once for all.

One of the few approaches to rates of conformational transitions in these short

time ranges is the measurement of ultrasonic absorption. In aqueous polyglutamic acid a relaxation corresponding to the helix-random coil transition was observed, and showed a maximum at the pH of the transition midpoint, as demanded by Schwarz's theory. Other ultrasonic absorption results, however, were too hastily interpreted and proton transfer processes were wrongly taken for the helix-coil transition. Zana and Lang (*Biopolymers*, **12**, 79; 1973) have now done measurements on polybenzyl-aspartate in a mixed solvent system, and find that though the level of absorption is different in the α -helical and random states—presumably in consequence of solvation differences—no maximum of absorption, such as would correspond to the transition process itself, is to be observed in the working frequency range. This makes it possible to set a lower limit on the relaxation time of the transition. The corresponding rate constant is higher by at least an order of magnitude than that observed for polyglutamic acid in water.

The various limitations in all these methods should direct all the more attention to an entirely new and potentially very versatile approach to fast kinetics. Feher and Weissman (*Proc. US Nat. Acad. Sci.*, **70**, 870; 1973) have evolved a technique for the study of the concentration fluctuations which prevail in a reacting system at equilibrium. The beauty of this method is that the equilibrium is not disturbed in the course of observation. It has been tried out on a system in ionic dissociation equilibrium, using the solution conductivity as the vehicle of observation. Feher and Weissman have succeeded in separating true concentration fluctuation noise from electronic and other types of

noise, and have extracted a frequency spectrum for the fluctuation. The temperature dependence yields a linear Arrhenius plot, and an activation energy agreeing precisely with the value from relaxation measurements. Inasmuch as conformational processes involving macroions, such as nucleic acids and proteins, are in general accompanied by changes in counterion binding and therefore solution conductivity, this approach may well be one of the brightest prospects now on the horizon.

INFLUENZA VIRUS

Human Pandemic Strains

from our Cell Biology Correspondent

IN 1968 the so-called Hong Kong strain of influenza virus arose in China and proceeded to cause a worldwide epidemic of influenza. The origin of Hong Kong influenza virus has remained something of a mystery, but two virologists, Laver and Webster, who have a bent towards molecular detective work, believe they may at last have uncovered the origin of this virulent virus (*Virology*, **51**, 383; 1973).

Last year Webster and Laver reported that the haemagglutinin subunits of the 1968 Hong Kong virus and preceding A2 Asian influenza viruses were completely different both in their immunology and their amino acid sequence. These observations made it highly unlikely that the Hong Kong virus was simply a mutant of some pre-existing human influenza virus. On the other hand, in 1968 and 1969 others had shown that the neuraminidase subunits of the Hong Kong virus were antigenically identical to those of preceding A2 Asian influenza viruses. These two

Sequencing of Sticky Ends

MANY coliphages, the most familiar example being lambda phage, contain a double stranded genomic DNA molecule with what are known as cohesive or sticky ends. The two ends of such molecules are single stranded and mutually complementary in base sequence so that the linear molecules can form circles, the two single stranded tails base pairing with each other, or can polymerize to form a linear or circular concatamers. The base sequences of the sticky ends of the lambdoid phages are apparently similar because DNA of phages of this family form mixed dimers, the single stranded tail of one phage DNA base pairing with that of its relative. Likewise another family of temperate coliphages, which include phages 186, P2 and 299, show a similar relationship with each other but not with the lambdoid phages. Murray and Murray have directly con-

firmed these relationships by analysing the base sequences of the sticky ends of five lambdoid phages and three other temperate phages. They report their results in *Nature New Biology* next Wednesday (May 30).

Their painstaking analysis, which testifies to the considerable progress that is being made in the development of techniques for sequencing directly DNA molecules, proves that the cohesive ends of the five lambdoid phages studied have identical nucleotide sequences. The sequences of the sticky ends of phages 186, P2 and 299 by contrast are not identical but they are similar, and sufficiently so for the DNAs to cohere in spite of regions of mismatch. These data indicate that during the evolution of these phages the sequence of the sticky ends has been conserved no doubt because of the part they play in the life cycle of the phages.

sets of observations led Laver and Webster to the conclusion that the Hong Kong virus might have arisen by recombination between an existing human virus and an animal or avian virus; the first donating neuraminidase subunits, the second donating haemagglutinin subunits to the recombinant Hong Kong virus.

In 1968 and 1969 it had also been shown that two strains of influenza virus that in 1963 had been isolated from ducks and horses were serologically related to the human Hong Kong virus and that this relationship was probably mediated only through the haemagglutinin subunits of the three viruses, avian, equine and human, because the neuraminidase subunits of the three proved to be quite unrelated.

What Laver and Webster have now shown is that the duck, equine and Hong Kong influenza viruses do indeed have haemagglutinin subunits that are serologically cross-reacting. Furthermore, by comparing peptide maps of the haemagglutinin subunits of the three viruses they have established that the light polypeptide chains of the subunits are virtually identical whereas the heavy chains differ significantly. It seems, therefore, that the Hong Kong virus might well have arisen by recombination between a human virus and an equine (or avian) virus related to equine 2 (or duck/Ukraine) influenza virus, and Webster and his colleagues have apparently obtained such human/animal (or avian) recombinants in experiments which mimic natural conditions of transmission.

Downie and Laver (*ibid.*, 259) also believe that there are probably plenty of as yet undetected avian influenza viruses in existence from which human pandemic strains may arise at any time as a result of such recombination. Indeed, they have isolated such a virus from an apparently healthy Australian shearwater. This avian virus is a type A virus, and of course pandemics of type A viruses occur in man. The haemagglutinin subunit of this shearwater virus belongs to a known avian type, but the neuraminidase subunit of the virus is antigenically distinct from all other known avian influenza viruses.

This virus is only the second type A influenza virus ever to have been isolated from a wild bird; it did not apparently cause any disease in its host and it has a novel neuraminidase subunit. Presumably many wild birds harbour infections by benign influenza viruses and they probably provide a pool of viruses which by recombination with existing human viruses can give rise at any time to new virulent strains able to cause pandemics in man. In short, epidemics of influenza seem certain to continue to plague man for the foreseeable future.

ANIMAL BEHAVIOUR

Aggressive Anemones

from our Animal Behaviour Correspondent

AGGRESSIVENESS is not a trait very obviously associated with sea anemones. Yet Francis (*Biol. Bull.*, **144**, 73; 1973) has not only found overt aggression in the Californian anemone *Anthopleura elegantissima*, but also produces evidence that they are selectively aggressive to anemones of a different genotype from themselves.

A most striking feature of the spatial distribution of the anemones is that they live in clumps: there are definite patches of animals interspersed with anemone-free spaces. A given clump is made up of individuals all of the same colour pattern and of the same sex. The anemones are known to reproduce asexually by longitudinal fission, so that the different clumps are almost certainly groups of genetically identical individuals (clones). When taken into the laboratory and mixed together, the anemones segregate back into their clones.

Contact between genetically different individuals was found to lead to an elaborate series of aggressive behaviours, one or both animals being damaged in the process. First, the tentacles touch and withdraw repeatedly. If the anemones are from the same clone, the animals move toward each other, their tentacles interlacing. But if they are

from different clones, this touching and withdrawing of tentacles is followed by distension of the acrorhagi — rounded objects situated beneath the outer layer of the tentacles. The inflated acrorhagi (now cone-shaped) are drawn upwards and then swept downwards towards the victim and wiped against its body. Tissue touched by the acrorhagi is damaged by the stinging cells they contain and begins to deteriorate. The victim either moves away or, if it is unable to do so, is killed.

Anthopleura shows this behaviour neither to species upon which it preys (for example, small mussels) nor to its predators (for example, sea slugs) and the curious clumped distribution of these anemones over the rocks would seem to be attributable to their own aggressive behaviour which has the effect of spacing them out from other anemones of alien genotype.

PLASMAS

Latest with Lasers

from a Correspondent

A CONFERENCE on laser produced plasmas, organized jointly by the Quantum Electronics and Plasma Physics Groups of the Institute of Physics, was held from April 17 to 18 at the University of Hull. The keen interest shown in the subject fully reflects the rapid increase in international activity since last year's publication of some

Shutoff Mechanism after Phage Infection

SHUTOFF of host transcription in *Escherichia coli* cells after phage T4 infection is prevented by chloramphenicol and it has been assumed therefore that a T4 protein(s) synthesized after infection is responsible for shutoff. But attempts to find the presumptive gene(s) which code(s) for such T4 protein(s) have been unsuccessful. The problem could be solved by study of a mutant defective in host shutoff. An RNA polymerase mutant (*Rif^R-2*) has been found which makes host RNA as late as 6–8 min after infection, and in next Wednesday's *Nature New Biology* (May 30), Snyder describes experiments with this mutant which cast a new light on the possible mechanism of host shutoff in phage infected cells.

Snyder investigated differences in sedimentation rate and efficiency of utilization of various templates in the RNA polymerase of infected and uninfected cells. These differences appear very early in wild type infection, but are delayed after infection of mutants in which shutoff of host transcription is also delayed. The effect is independent of chloramphenicol treatment.

The RNA polymerase from uninfected

cells sediments as a broad peak in glycerol gradients. Different parts of the polymerase peak transcribe different templates with varying efficiency, showing that the polymerase peak is heterogeneous. In particular, a rapidly sedimenting polymerase activity disappears quickly after T4 infection and the polymerase peak becomes more homogeneous. In the *Rif^R-2* mutant, however, the change in the profile of polymerase activity is delayed and even 8 min after phage infection the rapidly sedimenting activity can still be detected. This difference between the polymerase activity after infection of the mutant and wild type cells is independent of chloramphenicol treatment.

These experiments were carried out on crude preparations of RNA polymerase. Snyder suggests that the rapidly sedimenting polymerase may result from weakly bound factors which would be lost during the usual purification procedures and thus have not been detected previously. Thus he proposes that host shutoff is caused by inactivation of these factors which are required for transcription of host DNA *in vivo* but not *in vitro*.

American thermonuclear computations, which indicated that a laser fusion reactor is theoretically possible (see for example Nuckolls *et al.*, *Nature*, **239**, 139; 1972).

For many physicists, the computer dominated this meeting also. After a review of laser fusion calculations by Dr K. V. Roberts (UKAEA, Culham), Dr D. E. T. F. Ashby detailed recent results obtained with the Culham MEDUSA code. This spherical "one-dimensional" Lagrangian code neglects degeneracy and radiative reabsorption effects, but does include D-D, D-T and D-³He thermonuclear reactions; fusion neutrons are assumed to escape from the plasma, and charged reaction products deposit their energy locally. Assuming heat transport by electron conduction, this relatively simple model predicts thermonuclear gains of ~ 55 when 140 kJ of 10.6 μm radiation is absorbed by a 250 μg DT pellet, a result in good agreement with the work of Clarke *et al.* (*Phys. Rev. Lett.*, **30**, 89; 1973).

There is, however, an upper limit to the power density which can be transported by collision-free electrons; when this "free-streaming" limitation is introduced in an approximate manner to a previously optimized calculation, the thermonuclear yield is predicted to drop significantly. This new result is sensitive to wavelength, for it is postulated in the MEDUSA code that incident light not absorbed by inverse bremsstrahlung in the tenuous corona is subsequently absorbed at the critical density. (During the subsequent discussion it was suggested from the floor that the predicted decrease in yield could be alleviated by modifying the laser pulse shape to correct for the finite thermal transport time between the absorbing corona and the compressed core.)

Laser wavelength effects were also discussed by Dr G. J. Pert (University of Hull); "one-dimensional" numerical calculations indicate that of an incident 1.06 μm , 60 kJ laser pulse, only 1 kJ is absorbed by free-free absorption processes. Parametric and stimulated scattering processes are normally expected to absorb much of the remaining energy; however, it was claimed that at very short wavelengths free-free absorption would be too weak to compensate for the higher threshold intensities expected for anomalous absorption. Finally, Drs J. A. Wesson and J. E. Crow (Culham) discussed model compression experiments, in which the principle of adiabatic compression by pulse shaping could be demonstrated and heat transport and laser-plasma interaction effects tested using currently available 1–10 GW CO₂ lasers.

Excellent reviews on the physics and technology of ultraviolet, CGE Nd-glass, and CO₂ lasers were given by Professor D. J. Bradley and Dr M. H. Key (Uni-

versity of Belfast), Dr J. Guyot (Laboratoire de Marcoussi) and Dr D. J. James (University of Hull). These talks provided a good introduction to the real world of laser plasma experimentation and gas breakdown phenomena, topics which occupied more than half of the contributions. An understanding of gas breakdown phenomena is of topical interest in the design of high power molecular gas lasers; it is therefore noteworthy that Professor C. Grey Morgan (University College of Swansea) concluded his lucid review of this subject by observing that there is to date no unified theory which can account for all the existing CO₂ and Nd laser breakdown measurements. This observation was subsequently substantiated by new 3 ns CO₂ laser breakdown data (Dr G. Hill, University of Hull).

The meeting concluded with a contribution from Dr I. J. Spalding (Culham) on the reactor implications of laser fusion. Assuming electrical excitation, lasers having a pumping efficiency of some 15–30% and capable of generating thermonuclear gains exceeding 30, are required for economically attractive reactor applications.

Overall, the meeting left one with the impression of a wide range of interest and of expertise within British universities and research establishments. Many relevant experiments are possible with laser energies of 0.1–10 kJ and intensities of 10^{10} – 10^{16} W cm⁻².

Simple Models for Intramolecular Interactions

INTERACTIONS between acidic proteins and nucleic acids occur in a variety of interesting biological circumstances but the details are little understood. Novak and Dohnal (see *Nature New Biology* next Wednesday, May 30) have used the tactic of investigating the interactions of simple model compounds.

Starting with the supposition, for which there is growing experimental evidence, that strong interactions between purines and aromatic amino acids can occur, Novak and Dohnal manufactured the pentapeptide TGTGT where T is tyrosine and G glycine. This compound, they found, increased the T_m of calf thymus DNA by 6°, an amount significantly larger than that produced by basic peptides such as Arg₄. Moreover, the DNA peptide mixture was hyperchromic and this, they suggest, may result from the tyrosyl chromophores being introduced into a hydrophobic environment.

In support of this Novak and Dohnal found that the absorbance at 265 nm of TGTGT doubled when the peptide was transferred from water to dimethylsulphoxide. By contrast, they found that the analogous peptides TProTProT, in which rotation about the C–N bond is impeded, and the acidic peptide

REMOTE SENSING

Earth Gazing

from a Correspondent

THE first Earth Resources Satellite is in space, orbiting tropical and temperate latitudes and systematically recording by remote sensing systems details of the Earth's surface. Some of the instruments, problems, costs and benefits of obtaining, analysing and applying remote sensed imagery were discussed at a three-day symposium held at University College, London, between April 10 and 12 and sponsored by the British Interplanetary Society.

The symposium was less confined than the title would suggest, as many of the contributions and much of the discussion dealt with Earth observation platforms other than satellites. Several contributions were concerned with the use of the British Skylark rocket as an airborne platform for remote sensing. Dr N. Simmons (Ministry of Defence) assessed the relative merits of various airborne platforms, principally aircraft, rocket and satellite, and then dealt in detail with the particular merits of the operational Skylark rocket system.

Several contributions dealt with sensor technology, some with instrumentation and others with management and development problems and proposals. Dr O. Hofmann (Messerschmitt-Bölkow) outlined a new and quite novel multispectral vidicon scanner

TGluTGluT had no effect on the T_m of DNA.

The effect of TGTGT was not confined to DNA, for it also stabilized poly(rA):poly(rU) and poly(dG):poly(dC) which have secondary structures differing from that of DNA; by contrast, the peptide did not stabilize poly(dA-T):poly(dA-T) or poly(dA):poly(dT). These observations clearly do not admit of a simple explanation for the specificity of the stabilization, but some specificity for polymers containing G–C received support from precipitation experiments. Dinucleosides containing G were quantitatively precipitated by TGTGT at a ratio of one peptide to one dinucleoside. Dinucleosides which do not contain G were not precipitated.

Further experiments showed shifts in the NMR spectral lines corresponding to protons in both TGTGT and poly(A) in a mixture of these, and these shifts were all consistent with the tyrosyl residues intercalating between the purines—presumably thus placing themselves in a hydrophobic environment. This notion has obvious attractions, and it will be interesting to see if this proposed mechanism can account for interactions in real biological systems.

which operates without any moving parts—a great asset for remote operation.

Mr J. Plevin (Space Application Division, ESRO) presented a very reasoned case for the provision of an aircraft facility with advanced sensors, for European Earth resource scientists, in preparation for a European satellite programme. Even if a European satellite does not materialize there is still an urgent need to provide these facilities for the research scientists and in view of the costs involved the idea of a "European" facility is most rational and economic.

Mr S. L. Entres (Royal Aircraft Establishment, Farnborough) presented a masterly paper on the applications of orbital remote sensing in which he illustrated the efficiency of the system for the rapid collection of huge masses of information and showed that even at "pilot project", "evaluation" levels people are being drowned in data. He stressed that much more effort must be devoted to data handling and analysis, especially in developing automated systems. This, he argues, should take priority in current and near future remote sensing activity. Professor J. Hannessian (National Science Foundation, Washington) generated considerable interest with his contribution on the international and legal aspects of Earth observation satellites. Aircraft surveys over "alien" territory clearly violate national boundaries, but how high does a remote sensing platform have to be before it can be considered outside national boundaries.

The aim in space and airborne remote sensing operations is, of course, to apply the information collected to a specific problem. The "applications" groups were well represented and were drawn from a wide range of earth and environmental sciences. Mr N. P. Press (Royal School of Mines) spoke about the detection of the toxic effect of metals on vegetation and exploded the widely held (and printed) myth of the relationship between plant disease and chlorophyll content in the analysis of false colour photography.

Two contributions dealt with the problems of producing maps from satellite imagery. Dr O. Kolbl (Swiss Forest Research) was concerned with improving existing maps by the combined restitution of aerial and satellite photographs, and Mr P. G. Mott (Hunting Surveys) presented the viewpoint of the photogrammetrist that enough ground control exists, or could easily be obtained, to permit small scale mapping of the unmapped territories of the world.

Atmospheric pollution at local, national and international levels is a serious and growing problem and there is an urgent need to develop efficient recording and monitoring systems. In

this context Dr A. R. Barringer (Barringer Research Ltd) is one of the world's pioneers and he gave a fascinating account of some of the instruments and techniques he is developing for the remote measuring of certain types of atmospheric pollution.

The applications of remote sensing in climatology, vegetation, geology and hydrology were also considered and to illustrate further the broad interdisciplinary nature of the topic Dr W. G. Collins (University of Aston) outlined some of the work which is being carried out in the Remote Sensing Unit where the nine research students are drawn from seven different disciplines.

The symposium highlighted, and several contributors referred to, some of the most glaring defects in the remote sensing activities in Britain: its excessive fragmentation, poor communication and lack of any focal point.

Remote sensing involves instrumentation development, software development and applications, each of which, particularly the latter, cuts across the traditional discipline boundaries. This has resulted in a fragmentation of remote sensing activities in all agencies—universities, research councils, government departments and learned societies. Universities can do little by themselves (although the University of Aston has made a promising start) and although the research councils claim an interest in interdisciplinary research this has not been reflected in their support of interdisciplinary remote sensing research.

GEOPHYSICS

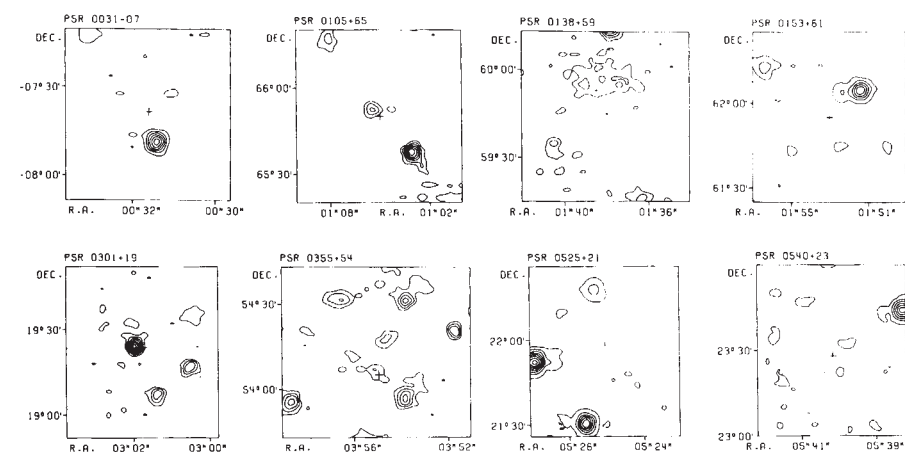
Volcanomagnetism

from our Geomagnetism Correspondent

OF the many potential methods of predicting seismic and volcanic activity, few can have proved more promising in theory and yet more discouraging in practice than that based on tectonomagnetism. It is now well established that when a stress is applied to, or removed from, a rock a small change in the remanent magnetization of the rock arises from the distortion of crystal structure (the piezomagnetic effect). Seismic and volcanic events reflecting changes in subsurface stress within the Earth should therefore manifest themselves as changes in the small part of the local geomagnetic field arising from the permanent magnetism in rocks. Continuous monitoring of the field should then lead to the observation of geomagnetic variations which reflect premonitory stress changes and thus possibly to a prediction of the time and location of an event.

Unfortunately, the phenomenon expected on the basis of theory and laboratory experiment is not easily observed in the field. For one thing, until recently, there have been practical problems associated with the measurement of very small field changes, distinguishing between the piezomagnetic signal and the much greater geomagnetic noise, and the insulation of magnetometers from mechanical vibration.

Continuum Radio Emission from Vicinity of Pulsars



THE suggestion that pulsars might possess weak radio haloes has been investigated by Schönhardt (see next Monday's *Nature Physical Science*, May 28). His results are disappointing, in that they show no evidence to support the hypothesis, recently put forward by Blandford, Ostriker, Pacini and Rees (*Astron. Astrophys.*, **23**, 145; 1973). But they provide an important contribution to the general pool of knowledge about pulsar properties.

In this figure, eight of the nineteen radio maps prepared by Schönhardt are shown. Each map covers about one square degree, centred roughly on the appropriate pulsar. Measurements were made with the Max-Planck 100 m, equipped with an 11 cm cooled parametric amplifier; contour intervals vary from one pulsar to another, but these eight examples are typical in showing no shell sources associated with the pulsars.

Thus although correlations between geomagnetic changes and seismic activity have been reported quite frequently since 1799 (von Humboldt), few, if any, can be considered genuine. The problems of sensitivity and vibration have now been largely overcome in modern equipment; and noise from distant sources such as the ionosphere can be filtered out by coupling local magnetometers with reference stations away from the zone of local tectonic activity. Even so, few valid examples of geomagnetic change related to stress have been reported, although the expected magnitude of the effect is as high as a few gamma.

Perhaps the best known success is the correlation between local magnetic changes and creep events on the San Andreas fault observed by Breiner (thesis, Stanford University; 1967), even though, at the same time, he failed to detect any relationship between geomagnetic variations and earthquakes in the same region. More recently, Davis and Stacey (*Nature*, **240**, 348; 1972) have further demonstrated that "the piezomagnetic effect is observable on a tectonic scale" by observing geomagnetic changes associated with the increasing water load in the Talbingo Dam of south-east Australia. Convincing observations of local magnetic changes preceding and accompanying eruptions of the Ruapehu and Ngauruhoe volcanoes of New Zealand have also been reported by Johnston and Stacey (*J. Geophys. Res.*, **74**, 6541; 1969, and *Nature*, **224**, 1289; 1969). On the other hand, Yokoyama (*The Surveillance and Prediction of Volcanic Activity*, UNESCO; 1971) has attributed geomagnetic changes associated with Japanese volcanoes to local changes of temperature rather than to stress.

In an attempt to find further examples of the volcanomagnetic effect, Davis *et al.* (*Science*, **180**, 73; 1973) have investigated Kilauea Volcano in Hawaii. On the face of it, Kilauea offers a good opportunity for this type of work because it frequently erupts, highly magnetic basalts surround it, and the existence of the Hawaiian Volcano Observatory close by gives a good "logistic basis" for an experiment involving repeated observations of an instrumental array. Davis and his colleagues thus constructed a linked array of three total field magnetometers comprising one local station close to Kilauea and two more distant reference stations (10 km and 43 km away, respectively).

Recording of the geomagnetic field was continued for a (broken) period of 12 months in conjunction with the existing programme of tilt and strain measurement in Kilauea which has already demonstrated inflation of the summit before eruptions and deflation during flank eruptions. The geo-

magnetic changes to be expected from such tilts can easily be estimated. Thus a tilt of about 10^{-4} rad, implying a strain of about 10^{-4} , would give a stress of about 30 bar in basalt, assuming perfect elasticity. Titanomagnetite of the composition indicated by the relevant Curie points would then give an expected change in magnetization of 2 parts in 10^4 per bar of stress, which corresponds to an absolute change of at least 3×10^{-5} e.m.u. and a resulting magnetic anomaly of the order of 10 gamma if the basalt is magnetic to a substantial depth.

But although such magnetic changes were observed in one of the experiments, they failed to correlate directly with the tilt records, and in a second experiment, no magnetic anomalies developed at all. Thus when the local station was at Uwekahuna, close to the Hawaiian Volcano Observatory, brief excursions of the magnetic field occurred. These

have a very slight correlation with small irregularities in the tilt records, but the effect is very localized. When the local station was moved to a site closer to the source of deformation, no magnetic anomalies developed at all even when tilting was violent.

The search for the volcanomagnetic effect from Kilauea was thus unrewarding, although even this negative result leads to a definite conclusion. It is clear that during a prolonged deformation of Kilauea the volcano cannot support any large scale pattern of shear stress but only transient, localized stresses accompanying local readjustments. Davis *et al.* do not discuss the effects of temperature at all; but it is interesting to note that their conclusion based on experiment was also reached by Yokoyama who argued the general case based on a consideration of temperature.

Scattering in the Lower Mantle

IRREGULAR structures acting as seismic wave scatterers in the Earth are generally difficult, if not impossible, to detect because the small amount of scattered energy is easily obscured by the primary wave. But in seismic shadow zones the conditions are more favourable. For example, in the PKP shadow zone caused by the presence of the Earth's core, the first phase to be expected on the basis of the most simple core model is PKIKP, the phase refracted through the inner core. In practice, however, precursors to PKIKP are observed in the approximate epicentral distance range 125° to 143° —a phenomenon for which an explanation is required.

In fact, there are several possible (though not mutually exclusive) explanations, including more complicated core models energy diffracted from the PKP caustic surface in a limited range near the caustic, scattering from the Bullen D" layer and in other parts of the mantle near the caustic surface, and scattering in the lower mantle beneath the source region. In next Monday's *Nature Physical Science* (May 28), Doornbos and Vlaar present an analysis of precursor data from the Norwegian seismic array (NORSAR) which provides evidence for all of these explanations except that involving a more complex core structure. In addition, they have been able to determine the locations of some of the inferred scattering zones.

From a spectral analysis of events in the Solomon and Fiji Islands regions during 1971 and 1972, Doornbos and Vlaar have been able to divide the precursors into three distinct classes based on differences in slowness ($dT/d\Delta$) and the azimuth of the direction of wave propagation. The first set comprises first arrivals in the range 143° – 138°

which have an average $dT/d\Delta$ value of about 3.3 s per degree and deviation from the true azimuth of less than 2° ; the second set includes phases with anomalously low $dT/d\Delta$ values in the range 1.6–3.0 s per degree and large azimuthal deviations in the range 10° to 40° ; and the third set consists of phases with anomalously high $dT/d\Delta$ values between 3.4 and 4.0 s per degree but azimuthal deviations within 5° .

There seems no reason to doubt that the precursors in the first set, at least those between 143° and 140° , are due to diffraction from the caustic as predicted by theory. Neither diffraction nor core complexity is, however, adequate to explain the second and third sets. If the phases in the second set are taken to be generated by scattering near the caustic surface, their characteristics may be used to determine the positions of the proposed scattering regions. Thus Doornbos and Vlaar conclude that scattering structures exist in the lower mantle at distances of 250 km, 500 km and 600 km from the core-mantle boundary. The errors on these locations are difficult to estimate, but are unlikely to exceed 100 km for the first one and 150 km for the other two. The uncertainties are thus not small but neither are they large enough to explain the depth spread of the locations.

The large $dT/d\Delta$ values for the precursors in the third set rule out an origin on the receiver side of the mantle but may be attributed to scattering structures on the source side. Accordingly, Doornbos and Vlaar also locate structures at distances of 200 km, 800 km and 900 km from the core-mantle boundary although in these cases the estimated errors amount to several hundred kilometres.

GEOPHYSICS

Mantle Plumes Get Cool Reception

"I DON'T believe them either, Harold," was how Professor Keith Runcorn comforted an incredulous Sir Harold Jeffreys at last week's meeting of the Royal Astronomical Society devoted to mantle plumes. Their scepticism was endorsed by almost all the attendance. The consensus was that there is little evidence for any of the mantle plumes which have been postulated to account for geological features on various parts of the Earth's surface, with the possible exception of the plumes held responsible for the Hawaiian islands and chains of seamounts in the Pacific.

Only one of the participants was more venturesome. Dr N. Petersen (Institute of Applied Geophysics, Munich) described work carried out with colleagues at Princeton University (see R. A. Duncan, N. Petersen and R. B. Hargraves, *Nature*, **239**, 82; 1972) as a result of which they identify two plumes at work on the European plate. The basic notion is of course that there are hotspots in the mantle which give rise to volcanic effects on the Earth's surface as the lithosphere moves over them, and it is an essential part of the hypothesis that the hotspots are comparatively narrow convection plumes. Apart from being a convenient way of accounting for the chains of volcanic features that litter the Earth's surface, these mantle plumes could conceivably initiate rifting and provide a mechanism for driving the plates. And if the hotspots can be considered fixed in the mantle—a qualification that may be difficult to sustain—then it ought to be possible to chart the movement of the plates by following the paths of the plume traces.

The two volcanic chains in the European plate which Dr Petersen and his colleagues suggest may be plume traces run in one case from the western part of the Eifel region in Germany through central Germany and Czechoslovakia to volcanic areas near Wrocław in Poland, and in the other case from Iceland to the Faeroe Islands, the islands of the west coast of Scotland, the Antrim plateau and the Mourne mountains of Ireland. The youngest ends of the two traces are at Iceland and the Eifel region, and Dr Petersen and his colleagues claim that the motions of the European plate deduced from each trace are consistent.

Criticism of this work centred on the Iceland plume, possibly because a decreasing age of the igneous activity from east to west in the central European chain is demonstrated only when the times of the most abundant and intense eruptions are considered. When the total duration of activity at volcanic centres along the chain is examined the evidence for a migration of activity

along the chain looks much less secure.

The chief point was that the evidence for migration of the Iceland plume along the section from the Mourne mountains to the Faeroes depends heavily on ages for the Mourne mountains and Antrim plateau which have recently been revised downwards so that they more nearly match the ages of Arran, Mull, Ardnarmurchan, Skye, St Kilda and the Faeroes. This age problem becomes more serious if the island of Lundy in the Bristol Channel is considered to be the end point of the chain. There is also the problem of explaining several igneous regions further out in the Atlantic such as Rockall which are well away from the postulated trace.

Dr Petersen's reply was to admit that the age relationships are not as good as he would have liked, but he continued to remain faithful to the Faeroes-Iceland sector of the Iceland plume and to the central European plume.

Having devoured Dr Petersen, the meeting turned to a consideration of how the required hotspots might arise. Professor B. A. Bolt (University of California, Berkeley) described how seismologists are beginning to be able to study the nature of the boundary between the mantle and the core, the interest here being the possibility mentioned earlier in the meeting by Professor R. Hide

(Meteorological Office) that bumps on the boundary could give rise to localized convective motions. Professor J. A. Jacobs (University of Alberta) described recordings by arrays of seismometers in Canada of earthquakes in the Tonga arc, the recordings suggesting the presence of inhomogeneities in the mantle beneath the Hawaiian islands. But Professor Runcorn (University of Newcastle upon Tyne) argued that the convection pattern from a hotspot on the boundary between the mantle and the core would not remain narrow but would spread out well before it reaches the surface. He felt that many people were imagining mantle plumes to be analogous to the narrow plumes of smoke which can develop in the atmosphere, whereas in fact the atmosphere is not a good model for behaviour in the mantle.

All the same, the meeting seemed prepared to concede that some kind of hotspot is responsible for the Hawaiian chain, even though there seems to be no firm evidence of mantle plumes at work on continental plates. But Professor D. L. Turcotte (Cornell University) cast doubt even on that. He suggested that the Hawaiian islands may be explicable in terms of a tensional fracture propagating in the lithosphere, causing volcanic activity as it migrates. The meeting on mantle plumes ended with Professor Jacobs's comment "We may be worrying about something that doesn't exist".

From Quantum Crystals to Neutron Stars

It is now generally accepted that pulsars—or at least most pulsars—are rotating magnetic neutron stars. It is therefore of interest to study the structure of neutron stars, and this raises problems in several completely different fields of physics. First of all, nuclear physics should ideally provide an equation of state, but nuclear forces are as yet not understood with sufficient accuracy to lead to a really reliable equation of state. Second, elementary particle physics should reveal something about the state of matter at densities which are so high that apart from neutrons, protons and electrons, other particles such as hyperons will appear. Again present knowledge about elementary particles is still far from complete, so that part of the theory of neutron star interiors is still chiefly a speculation. Finally, there are several problems which fall in what is broadly called solid state physics.

In early studies of the interior of neutron stars it was generally assumed that it would be a fluid—which might be both superfluid and superconducting. Recently several authors have suggested that the interior of a neutron star may

be solid. One such calculation is reported by Canuto and Chitre in *Nature Physical Science* next Monday (May 28). These authors apply a method, successfully used in describing quantum crystals such as ^3He and ^4He , and find that for densities above about $1.5 \times 10^{15} \text{ g cm}^{-3}$ the stable configuration is a face-centred cubic one in which the neutron spins show an "antiferromagnetic" ordering.

It is interesting to note that there is possibly some observational evidence in favour of a solid core. This evidence is provided by so-called glitches, the speedups of the Crab and the Vela pulsars, and the subsequent healing. These glitches have been explained in terms of star-quakes in which the interior of the neutron stars adjusts itself to the lower rotational velocity. Calculations by Pines, Shaham, and Ruderman (*Nature*, **237**, 83; 1972) have shown that to explain the magnitude and the frequency of the Vela glitches, it seems necessary to assume the existence of a solid core; this lends some degree of credibility to the results of calculations such as the one reported by Canuto and Chitre.

Metallic Ore Deposits and Continental Drift

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The application of plate tectonic concepts shows that ores associated with igneous activity must be concentrated in specific areas and allows determination of potential ore reserves from simple considerations.

THE history of the breakup of the continents during the past 200 m.y. is becoming increasingly clear^{1,2} and obviously pre-existing ore deposits which have been separated during this time can be matched across newly formed oceans³⁻⁵. This is significant in locating unknown reserves, for example in Antarctica⁶, but also means that techniques of exploration and exploitation which have proved most effective in one continent will also be the most efficient in matching regions. Locally such matching may be of direct economic importance, such as the predication of Pliocene copper ores in Mexico to match those of Boléo (Baja California) after reclosing the Gulf of California. Nonetheless, such matching is not of fundamental importance and becomes increasingly difficult for earlier times when the continental plates and metal provinces are more difficult to define.

Early concepts of the genesis of ores associated with igneous rocks were based on the assumption that metals segregated into the upper mantle and lower crust during the early cooling of the Earth and were remobilized and concentrated at the surface either by precipitation from waters heated by igneous intrusions or by direct magmatic differentiation. Clearly, any predication of potential reserves on this basis was largely empirical but at various levels of sophistication⁷. It had, for some time, been realized that certain ores were associated with particular geosynclinal conditions—although for unknown reasons. Recent studies of ore genesis where oceanic plates are being carried into the mantle along subduction zones⁸⁻¹¹, and where new oceanic crust is forming at the active oceanic ridges¹², allow the formation of ores to be considered throughout geological time in three principal environments—deep oceanic, subduction zone and stable continental block. As these environments have drastically changed their relative positions during at least the past 2,700 m.y., the determination of past continental relationships is fundamental to considerations of past ore formation. The past position of a continent relative to the Earth's axis of rotation can be obtained for any given time using palaeomagnetic methods¹³, and similarly the relative positions of different continental blocks can be obtained if

sufficient palaeomagnetic data are available for polar wandering paths to be compared—diverging paths indicating the blocks were then separated by a spreading ocean, converging paths requiring intervening subduction. These types of data also afford direct measurement of the palaeolatitude of the sampling sites, which is relevant to the formation of certain deposits.

Deep Ocean Environments

Mining of manganese-rich nodules which are strewn over the ocean floors^{14,15} may become economic in the near future, but their growth processes are not understood, so it is not clear how different plate environments on the ocean floor could influence their growth and composition. Near the active oceanic ridges, however, the sediments are locally enriched in metals^{16,17}, particularly copper and molybdenum, in areas marked by high heat flows. Similar enrichments have also been reported in older deep sea sediments¹⁸ formed presumably before these sediments were carried away from similar active parts on the flanks of the ridge, yet the volcanic activity at mid-oceanic islands is not accompanied by significant mineralization. This suggests that differentiation to give fluids rich in metals does not take place until after new oceanic crust has been carried away from the active magma injection zone. The localized high heat flows therefore mark areas where hot ore fluids are making contact with seawater in the overlying sediments with the resultant precipitation of copper sulphide, in particular, in a similar way to the genesis of the Kuroko copper sulphides of Japan¹⁹. This is presumably the way in which the massive pod and sheet copper sulphides of Cyprus were created^{20,21} in the Troodos Massif when it was part of an ocean floor, as these deposits are strongly concentrated towards the top of the oceanic igneous rocks. "Oceanic" ore deposits may also include podiform nickel, chrome, and so on²², formed by differentiation within the mantle and oceanic crustal rocks, or asbestos and laterites may form as a result of later chemical activity respectively before or after these rocks have been incorporated into a continent, such as during collision between different continental blocks.

The direct relationship between seafloor spreading and ore genesis was first clearly established in the Red Sea²³⁻²⁵ where hot, highly saline brines are presently depositing a variety of metal minerals as they mix with colder waters. Early isotopic analyses seemed to indicate that these metals were chiefly derived from the surrounding rocks by the leaching effect of brines heated by newly forming oceanic crust in the Red Sea, but some isotopic evidence²⁶ suggests the metals may be derived from the mantle. This is of academic importance but, irrespective of the source, similar deposition would exist along

any heated continental edge during initial separation. Most deposits formed during the past 200 m.y. have been covered by later sediments and are still covered by water. But some areas outcrop on land, for example, along the Nigerian Benue Trough which seems to have opened as the South Atlantic was opening²⁷, and mineralization along these areas may reflect deposits of the type found in the Red Sea.

Subduction Zone Environments

The successive occurrence of manganese, iron, porphyry coppers, gold, lead, zinc, silver and so on with igneous activity at increasing distances from oceanic trenches has been detailed, particularly for copper^{8-10,28,29}, in the Andes, Japan and the Philippines. The metals and minerals within the oceanic lithosphere carried down at subduction zones become molten at different temperature/pressure levels and therefore the composition of the magmas ascending to the surface from different levels varies away from the oceanic trench in both their major and minor elements³⁰⁻³³ and their accompanying hydrothermal fluids. Similarly the age of the magmas varies according to their fluidity, the depth of overburden and the availability of channels along which the magmas and fluids can ascend. Because the oceanic lithosphere thickness is remarkably uniform, the quantity of magma released, and therefore metals, is related directly to the rate of subduction. But the ability of these melts to reach the surface will be strongly controlled by the relationship between the fracture pattern within the overlying continental rocks and the direction of motion of the continental plate. There is a correlation between the location of orogenic episodes in the western United States with changes in the direction of motion of the North American plate³⁴ and these changes also correspond to times of peak mineralization^{35,36}. Further localization of volcanic, magmatic and metallogenic activity is controlled by the rare but thick aseismic oceanic ridges which, where they are being subducted, create compressive force on the overlying continental plate which closes the fracture pattern, preventing material rising to the surface from the subduction zone, particularly from depths less than 200 km. A notable example is where mineralization zones are restricted to the north and south along the borders of the Peru-Chile Trench where it intersects with aseismic ridges. The position of past intersections would vary with the direction of movement of the plates and the orientation of the ridge relative to the subduction zone. Such explanations are not exclusive of other restrictive factors. For example, the local geology into which the ore fluids intrude is significant in providing suitable physical and chemical environments in which the metal salts can be deposited. South American lead and zinc sulphides, specifically, tend to be locally restricted to areas where carbonate sediments are present²⁸.

This model of ore genesis can only be regarded as a first approximation in any given region because metallogenic provinces associated with subduction are complex, varying, for example, with the angle of descent of the oceanic lithosphere, which produces polymetallic areas where it is steep. Further complications arise where several subduction zones are simultaneously active within a single region, as in the West Pacific today, because these deposits may eventually become intermingled by the closure of marginal basins. Similarly, complexity can arise from repeated subduction at different times in the same locality, as in the case of Japan. Even greater complications may arise when previous subduction zones override a spreading ocean ridge, as in the western United States. Nonetheless, most metallogenic provinces along subduction zones of the past 200 m.y. or so can be explained by these fairly simple concepts and thus areas of ore genesis can be located by using, in particular, the palaeomagnetism of the ocean floors to measure changes in the direction and rate of motion of different plates. It is unlikely that the identification of much older subduction zones will be of such direct importance, as these orogenic areas have been subjected to erosion

and the metals, often only economically concentrated in the top 2-3 km, have mostly been removed, leaving the generally less economic root zones. But the location of previous subduction zones is of major significance, for they are probable source areas for some important sedimentary ore deposits.

Stable Continental Block Environments

Present plate theories suggest that away from plate margins the continental blocks behave as passive environments on which only sedimentation and erosion could take place, together with some vertical movements in response to mantle and lower crustal flow, for example during isostatic crustal readjustment. On this model, therefore, these environments are free from igneous activity, except for the carbonatites derived from the mantle. The carbonatites³⁷⁻³⁹, which include diamondiferous kimberlites, originate (by unknown mantle processes but seemingly related to continental separation) below the continental lithosphere, which is some 120 km thick by comparison with the 60-70 km thick oceanic lithosphere. The carbonatites are driven to the surface by an explosive blast of volatiles and, because of the accompanying degassing, contribute virtually no heat to the continental blocks. The formation of new ores in the stable block is, therefore, chiefly restricted to the redistribution and concentration of pre-existing ores. Archaean processes of greenstone/granulite segregations, or metals derived or generated by meteorite impact, are relevant sources, but I will not consider them here because plate tectonic concepts are not applicable to their origin.

Chemical erosion can remove soluble minerals, leaving residual concentrations rich in iron, aluminium, nickel and so on, depending on the composition of the rocks, so that the location of old oceanic or mantle rocks is of major consideration. Although chemical erosion is most active in humid, hot areas, palaeoclimatic considerations are generally not important as residual deposits formed in ancient tropical latitudes are readily eroded mechanically unless they are cemented, and, if cemented, are unlikely to be economic. Chemically inert minerals, containing gold, silver, tin, tungsten, and so on, may be removed in streams and deposited later as placers. The placer deposits tend to be richer nearer to the primary ore source so that they often form marginal to present day or old subduction and collision zones. More soluble minerals, such as the copper porphyries of a subduction zone, may also be carried short distances in arid climates, forming red bed coppers, such as those of the intermontane basins of the Andes²⁸, the Colorado Plateau⁴⁰ and the Fergana Basin (USSR), but such deposits, often containing uranium, vanadium and other heavy mineral concentrations, are susceptible to later chemical erosion and only deposits formed during the past 200 m.y. are likely to be still present in any quantity, unless preserved by impermeable rocks. Most metal sulphides, originally formed within subduction zones, are likely to be dissolved and carried away, mainly as chloride complexes and organogenic compounds, particularly if saline waters are available⁴¹⁻⁴⁶. This chemical removal of metallic salts from active or recently active subduction zones would normally be effectively complete in a short time, possibly some 10-20 m.y., and most solutions would be carried directly into the sea to become disseminated throughout the oceans. But some salts may enter environments where precipitation occurs and, under very specialized conditions, economically significant concentrations may form⁸.

Anaerobic conditions, especially with organic sulphur, seem to be a favourable geochemical environment on the continental block in which copper, lead and zinc sulphides are precipitated⁴⁷⁻⁴⁹. Such an environment could occur under a variety of circumstances, so it is necessary to consider the factors which optimize the conditions under which significant quantities of metal sulphides may be precipitated. Unfortunately such an examination must be based on the characteristics of known reserves as most geochemical factors are complex and

still inadequately known. Obviously the ground waters would be richest nearest to the original metal concentration, such as a subduction zone, and the content would tend to be highest where the highest salinities, temperatures and organic-chemical activity, that is, in low-latitude, arid areas⁵⁰, often closely associated with areas of very high organic content. The copper sulphides of the European Kupferschiefer beds, the Katangan-Zambian Copperbelt, the White Pine (Michigan) and Kazakhstan areas appear to have deposited in these low latitude conditions.

The origin of the Mississippi Valley type lead and zinc sulphides, in areas such as Tri State (USA), and the European Silesian ores, has long been in dispute^{47,51,52}. The absence or very small scale of obvious igneous activity or feeders and the sedimentary or early replacement nature of many of the stratiform ores indicate that they have not formed directly by igneous activity. The carbon, oxygen⁵³ and sulphur^{46,47,54} isotopes are of shallow origin and have been initially incorporated in the minerals at about the same time or shortly after the deposition of the host limestones. Saline inclusions within the ores, however, indicate crystallization temperatures around 100° C, but ranging between 50 and 200° C (ref. 56), and this has been interpreted to mean that igneous heating is necessary to warm metal bearing brines, even if igneous rocks are not necessary as a source for the metals. But ground waters rich in metals react with sulphur in organic-rich pockets and precipitate lead and zinc sulphides and the exothermic reactions within the organic material, especially between hydrocarbons and anhydrite^{55,57}, could cause localized high temperatures and the deposition of the sulphides itself appears to be strongly exothermic. Such temperature rises need be only small in low latitudes as the temperature of ground waters near the surface already falls within the lower temperature range.

Hydrocarbon-anhydrite reactions would also be associated with the low latitude⁵⁰ "sabkha" evaporite formations⁵⁸. Such an explanation for the genesis of these ores has several other implications because they should be approximately contemporaneous (within some 10 m.y.) with the subduction zones from which they originated and would be restricted to a comparatively short time interval when such rich ground waters were available. The ores would also be approximately contemporaneous with the limestones in which they are found because the organic reactions would be maximized shortly after the accumulation of the organic material, and the ores would therefore decrease with depth, reflecting the change in the original organic quantity and activity. The movement of ground water through the chiefly impermeable basement rocks would be strongly influenced by any fracture patterns and basement topography and such patterns would also influence the formation of solution cavities in the overlying limestones, which could fill with organic material in near-shore, shallow marine anaerobic conditions. Both factors would tend to concentrate lead and zinc deposits over basement "high" areas, particularly over intersecting fractures in the underlying basement^{59,60}.

Any of these ores may have been subjected to later metamorphism, structural dislocation⁶¹ and mobilization as later subduction zones or continental collisions occurred⁶², as in the Swedish and Australian major lead and zinc deposits and the copper ores of the African Copperbelt. Nonetheless, even in such complex areas, a knowledge of the conditions of initial formation and therefore the probable source rocks is important in planning future development, and the location of such ores in less complex situations can be assessed more readily from a knowledge of the location and age of past subduction and collision zones and of the palaeoclimatic conditions bordering these areas while they were being eroded.

Significance for Mining

The application of plate tectonic concepts within ore genesis has a variety of consequences. The principal feature is that

ores associated with igneous activity must be concentrated in specific areas at specific times and in a determinable order and age sequence. Such concepts are readily applicable to present subduction areas and allow determination of potential ore reserves from fairly simple considerations. For many other ore deposits the location of past subduction zones and slices of oceanic/mantle exposures are of critical importance but the most surprising consequence is that it affords an understanding of the conditions in which major sedimentary ores may have formed. This has been problematical for many decades and is important both for locating new reserves and exploiting known ore bodies. Such considerations need very much closer examination, particularly in testing the apparent low-latitude control of the deposition of such major ore bodies, and it is not to be expected that all ore bodies will fit into any simple mechanism⁶³. Nonetheless, the changing position of the continents in respect to subduction, collisions and palaeoclimate will clearly be of much greater importance in the future. As known resources become exhausted and technology changes, the economic factors change, so that poorer grades of ore will become workable. But exploration costs for the richer ores will also increase and it will therefore become essential to localize areas in which the more sophisticated and increasingly expensive exploration tools are applied. Perhaps more fundamentally, such considerations should allow a basis for evaluating the real potential and location of the Earth's remaining natural mineral resources.

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Natural Units in Atomic and Molecular Physics

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Professor McWeeny's account includes a list of natural units for the most common quantities in terms of four base units.

In theoretical atomic and molecular physics it is often convenient to use "atomic units". Such units have various advantages. In the first place they are "natural" units, provided by naturally occurring quantities such as the magnitude of the electronic charge (e) and the "rationalized" Planck constant ($\hbar/2\pi$), and are therefore independent of the practical units in terms of which their values can be assigned only by experiment: the charge on the electron does not depend on either measurement or international agreement, but its value in terms of the Coulomb does. Second, their adoption permits all the fundamental equations to be written in a dimensionless form in which constants such as e , m and $\hbar$ are absent and need not be considered at all during mathematical derivations or the processes of numerical solution; the units in which any calculated quantity must appear are implicit in its physical dimensions and may be supplied at the end. Although atomic units have been very widely used since the introduction by Hartree of the "double-Rydberg", and although the "hartree" and the "bohr"¹ have been quite widely adopted as units of energy and length, no complete discussion of natural units seems to be available in the literature. With the advent of the Systeme International (SI) d'Unites, the proposals of Shull and Hall¹ seem to merit reappraisal and extension.

In this article I shall give a fairly complete list of natural units for all the most commonly encountered quantities in terms of four base units, and I shall propose names for some of the units; the values of all of them are given here in terms of practical SI units. I shall also give two examples to illustrate the use of the units in reducing complicated equations to a simple dimensionless form.

Units

In the quantum mechanical treatment of the hydrogen atom (fixed nucleus approximation) there occur the quantities

$$a_0 = \hbar^2 \kappa_0 / m e^2, \quad E_0 = m e^4 / \kappa_0^2 \hbar^2 = e^2 / \kappa_0 a_0 \quad (1)$$

where a_0 is the radius of the first Bohr orbit and E_0 is the magnitude of the electronic energy in the ground state. The abbreviations used are $\hbar = h/2\pi$ (the rationalized Planck constant) and $\kappa_0 = 4\pi\epsilon_0$ where ϵ_0 is the permittivity of free space. These quantities are the SI equivalents of those for which Shull and Hall¹ proposed the names bohr and hartree

$$a_0 = 1 \text{ bohr (B)}, \quad E_0 = 1 \text{ hartree (H)}$$

The most convenient base units seem to be the mass of the electron (m), the magnitude of the electronic charge (e), the Planck constant ($\hbar$), and the permittivity (κ_0); in terms of these, the bohr and the hartree are "derived units". The natural units of all other physical quantities are easily expressed as dimensionally appropriate products of the form $m^p e^q \hbar^r \kappa_0^s$ where only integral powers occur.

Table 1 Base Units

Quantity and dimensions *	Natural unit and name †	SI equivalent ‡
Mass M	$m = 1 \text{ em (electron-mass)}$	$9.1091 \times 10^{-31} \text{ kg}$
Charge Q	$e = 1 \text{ e (electron)}$	$1.60210 \times 10^{-19} \text{ C}$
Action $MLT^{-1} (A)$	$\hbar = 1 \text{ P (planck)}$	$1.05450 \times 10^{-34} \text{ J s}$
Permittivity $Q^2 W^{-1} L^{-1}$	$\kappa_0 = 4\pi\epsilon_0$ (no name)	$4\pi \times 8.8542 \times 10^{-12} \text{ F m}^{-1}$

* The dimensions of all quantities are expressed in terms of the more conventional symbols M , L , T , Q . For convenience, A (action) and W (energy) are also frequently employed.

† The choice of m , e , $\hbar$ and κ_0 as base units fixes all others. There is no generally accepted set of names for the natural units.

‡ The SI base units required in these tables are the metre, kilogram, second and ampere (m , kg , s , A). The other important named units are the joule (J), newton (N), coulomb (C), watt (W), volt (V), farad (F), weber (Wb), henry (Hn) and tesla (T). (The henry is here denoted by Hn to avoid confusion with the Hartree atomic unit, H). For further information see ref. 3.

The base units of mass, charge, action and permittivity, and their values in SI units are indicated in Table 1, together with suggested names. The "planck" is obviously appropriate as the name of the unit of action; and the "electron mass" (or "em") and the "electron" (or "e") seem to be uncontroversial. The derived units are listed in three groups in Tables 2, 3 and 4, together with their dimensions in mass, length, time and charge (M , L , T , Q) and their SI values.

an atomic unit of magnetic moment, and a field of flux density 1 aT would be required to produce a free-electron ($g=2$) Zeeman splitting of 1 H.

Dimensionless Equations

In equations relating physical quantities it is customary to use the conventions of "quantity calculus" in which the symbols

Table 2 Derived Units (Mechanical)

Quantity and dimensions	Natural unit and name*	SI equivalent†
Length L	$\kappa_0 \hbar^2 / m e^2 = 1$ B (bohr, a_0)	5.29167×10^{-11} m
Time T	$\kappa_0^2 \hbar^3 / m e^4 = \hbar / E_0 = 1$ as ("atomic second")	2.41889×10^{-17} s
Velocity‡ LT^{-1}	$e^2 / \kappa_0 \hbar = a_0 E_0 / \hbar = 1$ B as $^{-1}$	2.18764×10^6 m s $^{-1}$
Force MLT^{-2}	$m^2 e^6 / \kappa_0^3 \hbar^4 = E_0 / a_0 = 1$ H B $^{-1}$	8.23831×10^{-8} N (J m $^{-1}$)
Energy $ML^2 T^{-2}$ (W)	$m e^4 / \kappa_0^2 \hbar^2 = e^2 / \kappa_0 a_0 = 1$ H (hartree, E_0)	4.35944×10^{18} J
Power $ML^2 T^{-3}$	$m^2 e^8 / \kappa_0^4 \hbar^5 = E_0^2 / \hbar = 1$ H as $^{-1}$	1.80225×10^{-1} W (J s $^{-1}$)

* The names bohr and hartree are fairly widely used for the units of length and energy. For the atomic unit of time the name "atomic second" is proposed, with the symbol "as" formed by adding the prefix "a" to the conventional symbol "s". Each unit is expressed first in terms of the primary units (m , e , $\hbar$, κ_0) of Table 1 and, wherever it is more convenient, in terms of a dimensionally equivalent combination of derived units.

† The expressions in parentheses indicate dimensionally equivalent units (for example, 1 N = 1 J m $^{-1}$) corresponding to those employed in column 2.

‡ In these units the velocity of light is 137.030 B as $^{-1}$, the fine structure constant being $\alpha = e^2 / \kappa_0 \hbar c = 1/137.030$.

Table 3 Derived Units (Electrical)

Quantity and dimensions	Natural unit and name*	SI equivalent†
Electric permittivity $Q^2 W^{-1} L^{-1}$	$\kappa_0 (=4\pi\epsilon_0) = 1$ e 2 H $^{-1}$ B $^{-1}$	$4\pi \times 8.8542 \times 10^{-12}$ F m $^{-1}$ (C 2 J $^{-1}$ m $^{-1}$)
Electric current QT^{-1}	$m e^5 / \kappa_0^2 \hbar^3 = e E_0 / \hbar = 1$ e as $^{-1}$	6.62329×10^{-3} A
Electric potential WQ^{-1}	$m e^3 / \kappa_0^2 \hbar^2 = E_0 / e = 1$ aV ("atomic volt")	2.72108×10^1 V
Capacitance $Q^2 W^{-1}$	$\kappa_0^2 \hbar^2 / m e^2 = e^2 / E_0 = 1$ e 2 H $^{-1}$	5.88774×10^{-21} F (C 2 J $^{-1}$)
Electric field strength (E) $WQ^{-1} L^{-1}$	$m^2 e^5 / \kappa_0^3 \hbar^4 = E_0 / e a_0 = 1$ aV B $^{-1}$	5.14220×10^{11} V m $^{-1}$
Electric displacement (D) QL^{-2}	$m^2 e^5 / \kappa_0^2 \hbar^4 = e / a_0^2 = 1$ e B $^{-2}$	5.72142×10^1 C m $^{-2}$
Electric dipole moment LQ	$\kappa_0 \hbar^2 / m e = e a_0 = 1$ e B	8.47778×10^{-30} C m
Electric polarizability $L^2 Q^2 W^{-1}$	$\kappa_0^4 \hbar^6 / m^3 e^6 = e^2 a_0^2 / E_0 = 1$ e B 2 aV $^{-1}$	1.64867×10^{-41} F m 2

* Each unit is expressed first in terms of the base units (m , e , $\hbar$, κ_0) of Table 1 and, wherever it is more convenient, in terms of a dimensionally equivalent combination of derived units. The "atomic volt" (aV) is proposed as a convenient unit of potential.

† The expressions in parentheses indicate dimensionally equivalent units (for example, 1 N = 1 J m $^{-1}$) corresponding to those employed in column 2.

The "bohr" and the "hartree" appear in Table 2 and are widely accepted already; the "atomic second" (1 as) has the dimensions of action/energy and could thus be written as 1 P H $^{-1}$ but it seems desirable to introduce a corresponding named unit and the addition of a prefix "a" to the symbol for the practical unit appears to be convenient and unobjectionable. As the fine structure constant $\alpha = e^2 / \kappa_0 \hbar c = 1/137.030$ it follows that a photon travels 137.030 B in 1 as.

In Table 3, the "atomic volt" (aV) has been introduced as the natural unit of potential; 1 hartree (27.2108 eV) of work is performed in moving 1 e between points differing in potential by 1 aV and hence 1 aV = 27.2108 V. The only other new name introduced (Table 4) is the "atomic tesla" (aT), for the natural unit of magnetic flux density. The significance of the unit may be visualized by reference to the Biot-Savart law: for unit charge (e), moving in a circular orbit, the flux density at the centre would be

$$\mathbf{B} = (\mathbf{r} \times e\mathbf{v}) / ((\kappa_0 c^2) r^3) = e\mathbf{L} / m(\kappa_0 c^2) r^3$$

For unit angular momentum (L) and unit radius (r) this would give a flux density (137.030) $^{-2}$ aT. The atomic tesla corresponds to an extremely strong field ($\sim 2 \times 10^5$ T), but fields in atoms may evidently be of the order 10^{-4} aT, or 20 T (that is 20 kgauss). The unit is convenient in discussing spin interactions: the Bohr magneton ($\beta = e\hbar/2m$) represents half

occurring stand for the quantities themselves: in this case dimensional consistency must be preserved in all mathematical manipulations and a term such as $\log(p/p_0)$ is acceptable (logarithm of a number) whereas $\log p$ (logarithm of a quantity, pressure, for example) is unacceptable. An alternative convention is to interpret the symbols as the numerical measures of the quantities they represent, referred to some specified system of units: in this case the equations contain only pure numbers or dimensionless variables; the ordinary processes of mathematical analysis apply without further reference to units, and the appropriate units are supplied at the end of a calculation, by reference to the physical dimensions of the quantity calculated. The second convention has much to recommend it and is tacitly accepted in atomic and molecular physics whenever atomic units are introduced, for example for convenience in computation. Here I simply give two examples to illustrate its advantages.

Electric Polarizability of the H Atom

With a field E along the z axis the unperturbed Hamiltonian operator ($H^{(0)}$) and the perturbation ($H^{(1)}$) are given by

$$H^{(0)} = -(\hbar^2/2m)\nabla^2 - e^2/\kappa_0 r, \quad H^{(1)} = Ee z$$

As any dimensionally consistent equation may be replaced by

Table 4 Derived Units (Magnetic)

Quantity and dimensions	Natural unit and name*	SI equivalent†
Magnetic permeability MLQ^{-2}	$\kappa_0 \hbar^2 / e^4 = ma_0 / e^2 = 1 \text{ em B c}^{-2}$	$1.87797 \times 10^{-3} \text{ N A}^{-2}$
Magnetic flux WTQ^{-1}	$\hbar / e = 1 \text{ P c}^{-1} = 1 \text{ aV as}$	$6.58199 \times 10^{-16} \text{ Wb (Vs)}$
Magnetic flux density (B) $MT^{-1} Q^{-1}$ (magnetic induction)	$m^2 e^3 / \kappa_0^2 \hbar^3 = \hbar / ca_0^2 = 1 \text{ aT ("atomic tesla")}$	$2.35055 \times 10^5 \text{ T}$
Magnetic inductance $WT^2 Q^{-2}$	$\kappa_0^2 \hbar^4 / me^6 = \hbar^2 / e^2 E_0 = 1 \text{ aV as}^2 \text{ c}^{-1}$	$9.93764 \times 10^{-14} \text{ Hn (Wb A}^{-1}\text{)}$
Magnetizing force (H) $QL^{-1} T^{-1}$	$m^2 e^7 / \kappa_0^3 \hbar^5 = E_0 e / a_0 \hbar = 1 \text{ H aT}^{-1} \text{ B}^{-3}$	$1.25165 \times 10^8 \text{ A m}^{-1} (\text{J T}^{-1} \text{ m}^{-3})$
Magnetic dipole moment $\ddagger L^2 QT^{-1}$	$e \hbar / m = E_0 ea_0^2 / \hbar = 1 \text{ H aT}^{-1}$	$1.85464 \times 10^{-23} \text{ J T}^{-1}$
Magnetic susceptibility $L^2 Q^2 M^{-1}$ (induced moment/flux density)	$\kappa_0^2 \hbar^4 / m^3 e^2 = e^2 a_0^2 / m = 1 \text{ H aT}^{-2}$	$7.89023 \times 10^{-29} \text{ J T}^{-2}$

* Each unit is expressed first in terms of the base units ($m, e, \hbar, \kappa_0$) of Table 1 and, wherever it is more convenient, in terms of a dimensionally equivalent combination of derived units. The "atomic tesla" (aT) is proposed as a convenient unit of flux density.

† The expressions in parentheses indicate dimensionally equivalent units (for example, $1 \text{ N} = 1 \text{ J m}^{-1}$) corresponding to those employed in column 2.

‡ The Bohr magneton ($\beta = e\hbar/2m$) corresponds to $\frac{1}{2}$ atomic unit of magnetic moment: the Zeeman splitting for a free electron ($g=2$) in a field of flux density 1 aT would be 1 H.

one in which all quantities have been replaced by their numerical measures, the corresponding dimensionless operators are (replacing $\hbar, e, m, \kappa_0$ by 1 throughout)

$$H^{(0)} = -\frac{1}{2} \nabla^2 - (1/r), \quad H^{(1)} = Ez \quad (2)$$

All symbols now refer to natural units (for example, the distance of the electron from the nucleus is r bohrs or ra_0). The unperturbed Schrödinger equation $H^{(0)}\psi = \epsilon\psi$ has a normalized ground state solution

$$\epsilon^{(0)} = -\frac{1}{2}; \quad \psi^{(0)} = (1/\sqrt{\pi})\exp(-r) \quad (3)$$

The first order perturbation $\psi^{(1)}$ satisfies the equation

$$(H^{(0)} - \epsilon^{(0)})\psi^{(1)} = -(H^{(1)} - \epsilon^{(1)})\psi^{(0)}$$

which becomes, in natural units,

$$(-\frac{1}{2} \nabla^2 - 1/r + \frac{1}{2})\psi^{(1)} = -Ez\psi^{(0)}$$

The solution is readily obtained in the form

$$\psi^{(1)} = -z(1 + \frac{1}{2}r)\psi^{(0)} \quad (4)$$

and the second order perturbation energy follows as

$$\epsilon^{(2)} = \langle \psi^{(0)} | H^{(1)} | \psi^{(1)} \rangle = -(9/4)E^2 \quad (5)$$

The electric polarizability α is defined by writing this result as $-\frac{1}{2}\alpha E^2$ and is thus $\alpha = 9/2$. The natural units of all quantities may be supplied from the tables, or from first principles. Thus polarizability is electric moment/field, where electric moment has the unit 1 e B and field the unit 1 aV = 1 H e⁻¹ B⁻¹ (field = potential gradient = energy $\times$ charge⁻¹/distance), and hence α has the natural unit 1 e B aV⁻¹ = 1 e² B² H⁻¹. In absolute units therefore (see Table 2 or equation (1))

$$\alpha = 4.5 e^2 a_0^4 / E_0 = 4.5 \kappa_0^4 \hbar^6 / m^3 e^6 = 4.5 \times 1.64857 \cdot 10^{-43} \text{ F m}^2$$

Any dimensionally equivalent statement may of course be used; for example (from equation (1)) $\alpha = 4.5 \kappa_0 a_0^3$.

Similar considerations apply to the wavefunction: the dimensionless r is distance/ a_0 and thus when r is reinterpreted as an actual distance the dimensionless r must be replaced by r/a_0 . Also $|\psi|^2$ is a probability density and has dimensions L^{-3} : the normalizing factor in conventional units thus has dimensions $L^{-3/2}$ and the dimensionless factor must be multiplied by $a_0^{-3/2}$. Thus

$$\psi(r) = (\pi a_0^3)^{-1/2} \exp(-r/a_0) \quad (7)$$

It is seldom necessary to revert to conventional units, but this can always be done, even in the most complicated cases, by use of the tables.

Terms in the Dirac-Pauli Hamiltonian

After reduction to Pauli form (see, for example, McWeeny and Sutcliffe²) the Dirac equation for one electron in the presence of an electromagnetic field ($\mathbf{E}$ and $\mathbf{B}$ arising from scalar and vector potentials ϕ and $\mathbf{A}$) may be written

$$H = \frac{1}{2}\pi^2 - e\phi + \frac{1}{2}g\mathbf{B} \cdot \mathbf{S} + g\alpha^2(\mathbf{S} \cdot \mathbf{E} \times \boldsymbol{\pi} - \mathbf{S} \cdot \boldsymbol{\pi} \times \mathbf{E})$$

Here natural units have been employed throughout, so all

symbols may be interpreted as the dimensionless measures of the quantities they represent; the operator $\boldsymbol{\pi} = \mathbf{p} + \mathbf{A}$ (where $\mathbf{p}$ is the usual momentum operator), α is the fine structure constant, and in Dirac theory $g=2$. Relativistic variation of mass has been neglected and an unimportant term in $\text{div } \mathbf{E}$ has been discarded as usual.

Suppose, having derived such an equation, or any results that follow from it, one wishes to supply the missing combinations of fundamental constants. When the symbols in equation (7) are reinterpreted as quantities, instead of dimensionless variables (that is their numerical measures in natural units), every term must take the overall dimensions of energy ($W = ML^2 T^{-2}$). Thus $[\pi^2] = [\text{momentum}^2] = M^2 L^2 T^{-2} = MW$ and the missing factor before the π^2 , whose dimensions are M^{-1} and whose measure in natural units is 1, must therefore be m^{-1} . Similarly, from Table 4, $[\mathbf{A}] = [\mathbf{B}]L = MLT^{-1}Q^{-1}$ and the missing factor before the $\mathbf{A}$ has dimensions Q . Thus $\mathbf{A} \rightarrow e\mathbf{A}$.

In the same way, if we use the common convention of retaining dimensionless angular momentum operators ($\mathbf{S}$), the $\mathbf{B} \cdot \mathbf{S}$ term has dimensions $MT^{-1}Q^{-1}$ and therefore requires a factor with dimensions $WM^{-1}TQ$. As action (A) = energy $\times$ time, this is equivalent to $AM^{-1}Q$ and the missing factor is $\hbar/m$.

Finally, as $\mathbf{E}$ has dimensions $WQ^{-1}L^{-1} = M^2 L T^{-2} Q^{-1}$, it follows that $[\mathbf{S} \cdot \mathbf{E} \times \boldsymbol{\pi}] = M^2 L^2 T^{-3} Q^{-1}$ and the dimensions of the missing factor must be $WM^{-2}L^{-2}T^3Q = M^{-1}TQ$. Because the natural unit of time is $\kappa_0^2 \hbar^3 / me^4$ the factor must be $\kappa_0^2 \hbar^3 / m^2 e^3$.

On putting these results into equation (7) and remembering that $\alpha = e^2 / \kappa_0 \hbar c$, one obtains the Hamiltonian

$$H = (\mathbf{p} + e\mathbf{A})^2 / 2m - e\phi + g\beta \mathbf{B} \cdot \mathbf{S} + (ge\hbar/8m^2 e^2)(\mathbf{S} \cdot \mathbf{E} \times \boldsymbol{\pi} - \mathbf{S} \cdot \boldsymbol{\pi} \times \mathbf{E}) \quad (8)$$

where $\beta = e\hbar/2m$ is the Bohr magneton and all terms are compatible with the use of SI units.

These examples are typical. Provided that SI conventions are respected, mathematical derivations and calculations may be performed entirely in terms of dimensionless variables, simply by discarding the constants $e, m, \hbar$ and $4\pi\epsilon_0$, which become the natural units of charge, mass, action and permittivity. The results are immediately expressible in practical (SI) units by reference to the tables given. The mixed gaussian system does not possess sufficient internal consistency to be used in a similar way, the use of two definitions of charge leading to ambiguities and the appearance of spurious powers of c .

I thank Dr D. A. Dunmur for reading this article and checking the tables.

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New Evidence for a Relationship between Tobacco Smoking, Nicotine Dependence and Stress

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Rats which have learned to avoid an electric shock under the influence of nicotine can become dependent on nicotine for a successful performance. Evidence is presented which suggests that this dependence is related to the degree of stress to which the animal is exposed.

It is now reasonable to assume that many inhaling cigarette smokers become dependent on the nicotine content of tobacco smoke, although why this dependence develops is not yet fully understood. Studies in experimental animals point primarily to nicotine and cigarette smoking having a stimulant action^{1,2}, and this could explain the subjective impression that cigarette smoking in man improves concentration and efficiency¹. The demonstration by Frankenhaeuser *et al.*^{3,4}, that cigarette smoking facilitates the performance and enhances the efficiency of habitual smokers in a behavioural situation characterized by monotony, would appear to support such a hypothesis. This seems unlikely to be the complete explanation for the development of the smoking habit, however, as dependence on nicotine probably results from exposure to a variety of emotional circumstances. On the one hand, the arousal effects of nicotine could improve concentration and therefore the overall capabilities of the smoker, enabling him to cope more satisfactorily with an allotted task without feeling anxious. This may explain the subjective impression of some smokers that they are calmed by smoking a cigarette. On the other hand, studies of the environmental circumstances which may contribute to the development of the smoking habit suggest that nicotine, in the medium of tobacco smoke, may help to alleviate the effects of stress mediated by psychological or physical discomfort. Smokers do appear to smoke, or to smoke more, when exposed to an emotionally stressful situation⁵; this could apply to 70% of heavy smokers and 30% of light smokers⁶. From the available evidence it seems that the experience of stressful situations can contribute not only to the beginning of the smoking habit, but also to its continuation and to the number of cigarettes consumed. We believe that the experiments reported here provide new evidence, albeit from animal studies, for a relationship between cigarette smoking, nicotine dependence and alleviation of stress.

Experiments were carried out with rats which were trained to avoid electric shocks by pressing a lever⁷. Rats have difficulty in learning this type of behaviour⁸, and the stressful nature of avoidance experiments has previously been demonstrated⁹. By modifying the experimental situation we showed that the effects of nicotine can be related to the degree of stress to which the animal is exposed.

Nicotine in a Stressful Situation

Male and female Sprague-Dawley and cross-bred (Sprague-Dawley/Lister black-hooded) rats were trained to press a

lever on a shock avoidance schedule. Sixteen pairs of rats of the same sex, strain and age were used, one animal receiving 0.4 mg nicotine kg⁻¹ subcutaneously immediately before each training session (nicotine-trained), the other receiving saline (saline-trained). The dose of nicotine, as the hydrogen tartrate, is expressed in terms of the base. During training, electric shocks lasting 0.5 s were delivered through the grid floor of the chamber every 3 s. Each lever-pressing response restarted the timer and so postponed the next shock for 30 s. Shock levels were increased during training from 0.1 to 0.4 mA. Some rats took longer to train than others. But when the rats had learned to avoid most of the shocks they were placed on a final schedule in which 0.4 mA, 0.5 s shocks were programmed every 30 s, each lever-pressing response postponing the next shock for 30 s. The first day on this final schedule was considered as the first day of the experiment. Each rat was run singly for 60 min daily at the same time for 5 d a week throughout both the training period and the experiment.

Saline was substituted for nicotine on the fourth or twelfth day of the experiment and this substitution was repeated at intervals of 1 week on three other occasions. Nicotine was given before the session on all other days. The saline-trained rats received saline before each session throughout the experiment.

At the beginning of the experiment the nicotine-trained rats pressed the lever more often and received fewer shocks than their saline-trained partners. During the first 3 d of the experiment thirteen out of sixteen nicotine-trained rats took fewer shocks than their partners ($P < 0.05$, Wilcoxon matched-pairs, signed-ranks test¹⁰). When saline was substituted for nicotine the number of shocks received by the nicotine-trained rats increased to such an extent that most took more shocks than did their saline-trained partners on the same day. The difference was not quite significant for the first two occasions on which nicotine was replaced by saline, but significantly more shocks were taken by the nicotine-trained rats on the third and fourth occasions ($P < 0.01$, Wilcoxon matched-pairs, signed-ranks test). It thus seemed that the nicotine-trained rats had become dependent on nicotine for their continued successful performance and that in its absence they coped less effectively with the situation than did rats which had never experienced the drug.

Fig. 1 shows the number of shocks received by two of the pairs of rats studied. In the first pair, there was a large and persistent disruption of behaviour when saline was substituted for nicotine, with an approximately four-fold increase in the number of shocks received. In the second pair, the nicotine-trained rat took progressively more shocks after repeated saline substitutions.

The simplest explanation for the poor performance of the saline-substituted rats is that they were unable to remember the response in the absence of nicotine. This has been referred to as dissociation of learning¹¹. Fig. 1, however, shows that it was possible for a rat to perform adequately during the first saline substitution—indicating that the behaviour was remembered—but then to give a progressively poorer performance after subsequent substitutions. Two pairs of nicotine-trained rats, in which saline was repeatedly

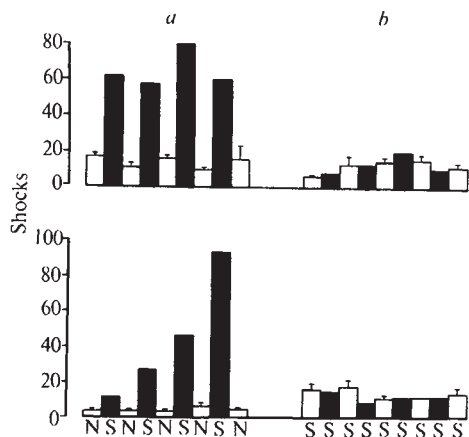


Fig. 1 Number of shocks received by two pairs of rats. *a*, Nicotine-trained; *b*, saline-trained. Solid columns, no. of shocks on four occasions at 1 week intervals on which saline was substituted for nicotine; open columns, mean shocks on the intervening days and on the 3 d preceding the first and following the last substitution with standard errors. S, Saline, N, 0.4 mg nicotine kg^{-1} before the sessions.

substituted for nicotine, still showed disruption of behaviour on the thirteenth occasion on which nicotine was withdrawn. This provides further evidence that disruption of behaviour was not caused by dissociation of learning, as these animals had the opportunity during this time to relearn the avoidance response in the presence of saline.

The disruption of behaviour which occurred after substitution of saline for nicotine was not confined to any particular strain or sex. The severity of the disruption did not seem to depend on the time at which saline was first substituted for nicotine, as withdrawal of nicotine on the fourth or the twelfth day of the experiment produced a comparable deterioration in performance.

A second series of experiments was performed to study the effects of withdrawing nicotine in a less stressful situation. In these experiments the avoidance schedule differed only in that a 10 s white-noise feedback stimulus was presented each time the lever was pressed. This "safety" signal reduces the stressful effects of an avoidance situation⁹. On the two occasions on which nicotine was withdrawn in these conditions the nicotine-trained and saline-trained rats did not differ in the number of shocks received. When the "safety" signal was discontinued, however, and saline substituted for nicotine on two further occasions, the performance of the nicotine-trained rats deteriorated.

From these experiments, it seems that rats which have learned, while under the influence of nicotine, to avoid an electric shock in the absence of any feedback signal, can become dependent on nicotine for an effective performance. The presence of a "safety" signal lessens this dependence, possibly by reducing the degree of stress to which the animal is exposed.

The stressful effects of unsignalled avoidance schedules have previously been demonstrated in rats and monkeys^{9,12}. Intensive training on an unsignalled avoidance schedule produced marked gastric ulceration in rats⁹, although little ulceration developed in animals which were given a brief feedback or "safety" signal after each avoidance response. In rhesus monkeys, studies of the neuroendocrine response to stress showed that although restraint, or lever-pressing for food, did not influence plasma 17-hydroxycorticosteroid levels, a four-fold increase in plasma corticosteroids occurred when the animals were placed on an unsignalled avoidance schedule¹². In contrast, when the avoidance procedure included an exteroceptive warning stimulus signalling the imminence of shock, the corticosteroid response was considerably reduced. When a "safety" signal was used in the experiments now reported, the degree of stress to which

the rats were exposed was likely to be considerably reduced, and this probably accounts for the somewhat minimal disruption of behaviour when saline was substituted for nicotine in these conditions. This evidence may serve to relate the degree of dependence on nicotine to a particular level of stress.

The pharmacological effects of cigarette smoking can be mimicked by a series of small intravenous injections of nicotine^{1,2}. After the first series of experiments, intravenous cannulae were inserted into the jugular vein of four of the nicotine-trained rats, the usual aseptic precautions being observed throughout the operative procedures. A "smoking dose" of nicotine (2 or 4 $\mu\text{g kg}^{-1}$) administered intravenously every 30 s for the 30 min preceding an experimental session substituted effectively for the usual subcutaneous dose of nicotine (Fig. 2).

Nicotine and Stress Mechanisms

Administration of nicotine to various species causes an appreciable and consistent increase in plasma¹³ and adrenal (unpublished results of D. J. K. Balfour) corticosteroid levels. It has been suggested that effective behaviour depends on some optimum level of stress¹⁴ and the development of successful avoidance behaviour in the rat has been shown to be accompanied by increased plasma corticosteroid levels¹⁵. Thus, if a facilitated adrenocortical response is necessary for effective behaviour, this might explain the development of dependence on nicotine after repeated exposure to an emotionally stressful situation. But recent studies have indicated that the initial increase in plasma corticosteroid levels which follows the intravenous administration of nicotine is not maintained even if the injections of nicotine are continued (unpublished results of D. M. Turner).

An alternative explanation for the mechanism through which nicotine alleviates the effects of stress is suggested by the results of recent experiments. Nicotine injected intravenously, or perfused through the third cerebral ventricle of the cat brain, causes an increase in the release of noradrenaline from the hypothalamus¹⁶. These observations are of particular interest, as the release of corticosteroids from the adrenal cortex in response to stress can be inhibited by infusing noradrenaline directly into the third cerebral ventricle¹⁷. In stressful situations, nicotine might therefore act by releasing noradrenaline from the hypothalamus, resulting in the inhibition of corticosteroid release from the adrenal cortex. To test this hypothesis we have carried out a preliminary series of experiments in the anaesthetized cat. We have shown that nicotine perfused through the hypothalamic region of the third cerebral ventricle can cause a reduction in circulating plasma corticosteroids. This effect is only observed if resting corticosteroid levels are relatively high as a result of surgical stress and exposure to

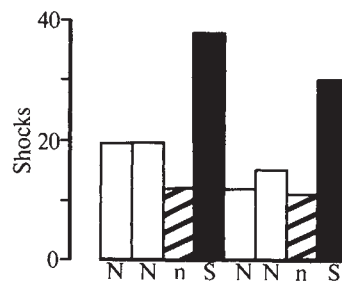


Fig. 2 Effect of intravenous nicotine on the number of shocks received by a nicotine-trained rat. Each column represents an experimental session. N, 5 μl saline intravenously every 30 s for 30 min followed by 0.4 mg nicotine kg^{-1} subcutaneously; n, 2 μg nicotine kg^{-1} intravenously in 5 μl saline every 30 s for 30 min followed by saline subcutaneously. S, Saline intravenously and subcutaneously.

anaesthetic. These preliminary findings offer additional evidence for a relationship between nicotine dependence and the alleviation of stress.

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Competition between Nerves for Functional Connexions with Axolotl Muscles

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This article provides a reinterpretation of the phenomenon of coordinated movement of grafted salamander limbs and suggests a mechanism for modifiable connexions between excitable cells.

SALAMANDER limbs implanted near those of a host become innervated and begin to move in time with movements of the limbs of the host animal. This ability was ascribed by Weiss to a message that ascended motoneurone axons and was able to readjust the impulse firing pattern of the motor nerve cells. Each motoneurone that grew a fibre into the grafted limb would have to change in some way so as to select from the central nervous system those excitations that were appropriate for the muscle in which the axon now terminated, a process which Weiss suggested could be called myotypic modulation. The other hypothesis to account for functional recovery of limb coordination, that of selective reconnexion of nerves with their own kind of muscle, was discarded by Weiss when he found that the pattern of innervation of the graft was not the same as that of the normal limb either in segmental origin or distribution to the muscles¹. Myotypic modulation or respecification of motoneuronal function was also proposed to account for the good recovery of coordination of the salamander's own limbs when the nerve supply was cut and allowed to regenerate in an apparently haphazard manner².

This is not, however, the mechanism for the excellent recovery of fish fin or eye muscle coordination after similar reinnervation experiments^{3,4}. Myotypic modulation has not been found, no readjustment of impulse discharge of motoneurons has been detected and muscles do become functionally reinnervated by their own motoneurons⁵⁻⁹.

We now have evidence that much the same thing happens in reinnervation of salamander limbs. Even after maximum disorganization of the nerve supply to the whole limb a high degree of order, replicating the original pattern of nerve-muscle connexion, is apparent in the end result of functional reinnervation.

The two principal nerve trunks supplying one hindlimb of axolotls (larval forms of *Ambystoma mexicanum*) were broken half way down the limb, pulled back central to the pelvic girdle, crossed and pushed back down the wrong side of the ischium. The gross misdirection of nerve trunks at the root of the limb was maintained for at least 9 months. Nerves regenerated from the central cut ends of the trunks, each one in the wrong part of the limb, and came to resupply the limb muscles. No attempt was made to aid regeneration by suturing or apposing nerves and no anatomical barrier at this level would guide or restrict the extent of nerve growth. The leg, which was initially completely paralysed, began to move in 8 weeks and had recovered by 3 months. All movements were in the normal direction. No sign of reversed movement was seen.

Animals were killed at intervals from 45 d to 9 months and the hind limbs set up for stimulating the peripheral cut ends of each of the spinal nerve roots and, after the first two experiments, for recording the evoked junctional potentials by intracellular micro-electrodes in the flexor musculature of the limbs. Up to 200 penetrations were made over the flexor surface of each leg and the response of each muscle fibre to stimulation of each of the spinal nerves that contributed to the limb was noted.

In this way a map of the distribution of functional segmental innervation of limb muscles could be made. All muscle fibres showed electrophysiological evidence of innervation by several nerve fibres but each segmental nerve territory in the musculature has a sharp border in that no muscle contraction was seen and no action potentials could be recorded from muscle fibres beyond it. The borders of some territories were contiguous and others overlapped. Normally the details of the pattern of segmental innervation of a pair of limbs in one animal are remarkably similar (Fig. 1). There are variations between animals as to which spinal nerves supply the limb and their exact muscle territories. For example, the eighteenth nerve sometimes contributes to limb muscle innervation as in Fig. 1 and sometimes it does not enter the leg at all.

After regeneration of crossed limb nerves the symmetrical pattern of segmental limb muscle innervation was restored (Fig. 2). In spite of being directed into the posterior aspect of the limb, motor fibres of the sixteenth nerve came to innervate anterior muscles, and the reverse was the case for the seven-

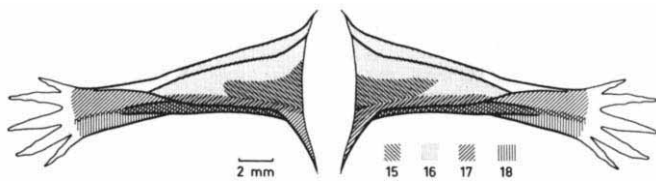


Fig. 1 Segmental innervation of muscles on the flexor surface of a pair of normal axolotl hind limbs. The map was prepared by stimulating the peripheral cut end of each of the spinal nerves 15–18, observing contraction and verifying areas of innervation by intracellular recording of evoked junctional potentials in muscle fibres. The limbs are seen from the ventral aspect with the anterior borders at the top. Note the symmetry of patterns of innervation. Scale is approximate.

teenth nerve. A small patch of muscle innervated by the fifteenth nerve was symmetrically disposed on each limb. This was the case in all animals (seven out of nine) allowed to survive long enough for good functional recovery, which we suggest could be largely a result of re-establishment of appropriate connexions between motor nerve fibres and muscles.

As each nerve trunk was initially directed into denervated muscle it is difficult to see how the growing nerve sprouts could avoid making their first contact with the wrong muscle fibres. Either they did not form neuromuscular junctions there or the ones they did form were trial synapses that did not remain functional. Provided nerve growth was profuse the eventual selection of the best set of nerve-muscle combinations could take place through interactions between a muscle cell and a number of convergent synaptic connexions more or less well-matched on embryological grounds, with the common post-synaptic muscle fibre. Competitive reinnervation of fish eye muscles by the original plus a foreign nerve results in the dominance of the correct innervation and a peripheral repression of the effectiveness of the competing innervation, without apparent degeneration or retraction of foreign nerve terminals⁹.

There was no behavioural evidence of cross-innervation of limb muscles in the course of recovery, but this could have been concealed by stronger contractions of correctly connected muscles. We made microelectrode recordings only from 175 d after nerve section, by which time movement was fully restored. That a competitive mechanism is operative in reinnervation of axolotl muscles was discovered in the following way.

Cutting one of the spinal nerves which supplied a salamander limb leads to an expansion of the territories of muscle innervation of the adjacent segmental nerves^{10,11}. We studied this process through two cycles of expansion, retraction when the severed nerve regrew into the limb and re-expansion when it was cut again.

Axolotls were prepared by cutting either the sixteenth or both the fifteenth and sixteenth spinal nerves on one side. Animals were allowed to survive for varying periods from 1 d to 6 months after which they were killed and the hind limbs mounted in a bath for electrical stimulation of the peripheral cut ends of the spinal nerves and microelectrode recording of junctional potentials in the flexor limb musculature. The pattern of innervation on the operated side, as shown by visual location of muscle contraction and confirmed by junctional potential recording, was compared with that of the opposite normal side. The limits of segmental territory were taken as areas in which maximal nerve stimulation at ten pulses per second caused no visible muscle contraction and no action potentials could be recorded by an intracellular microelectrode.

Immediately upon cutting a spinal nerve the dependent muscles were paralysed. Two or three days later the spread of muscle innervation first appeared as a marginal extension of the territories of adjacent nerve roots. The extension was only over about 0.5 mm but this was one-quarter to one-fifth of the width of the leg of the animals used and was clearly distinguishable from the pattern on the opposite leg. Muscle fibres in this area were innervated by terminals giving large junctional

potentials with normal latency and strong muscle contraction. Between 1 and 2 weeks later the adjacent segmental innervation spread further and patches of innervated muscle fibres were detected in the central denervated zone. Junctional potentials tended to be smaller and the latency from nerve stimulation longer (Fig. 3). Thirty days after nerve section both kinds of foreign innervation began to disappear as the original innervation grew into the limb. Eventually the normal symmetrical pattern was re-established.

Cutting the sixteenth root a second time in animals that had recovered completely from a previous denervation resulted in an area of paralysed muscle fibres in the expected part of the limb. During the next 2 d there were no changes in the boundaries of territories of the other nerves, just as after the first denervation. In the limbs of animals killed from then on, however, the whole of the expected denervated area was cross-innervated by adjacent roots. After the first denervation this amount of spread took 3 weeks, not 3 d. Junctional potentials were of normal size and latency. There was no further change of innervation pattern in animals examined over the next 2 weeks (Fig. 4).

We suggest that the rapid spread of innervation occurring 3 d after cutting a nerve, either the first or second time, is a result of the emergency of function in pre-existing but functionless nerve terminals.

In normal limbs spread of innervation by this mechanism is confined to the margins of segmental territories. The later, patchy spread, appearing within 1 or 2 weeks and extending into the centre of a denervated area, could be a result of true collateral sprouting of remaining nerves (Fig. 5).

After allowing complete regeneration of the one original cut nerve back into the limb all foreign innervation, including that due to collateral sprouting, is repressed leading to the re-establishment of the normal pattern. When the same nerve is cut the second time, even 6 months later, foreign innervation is derepressed within 3 d, but as foreign terminals are now widespread in the limb the second spread of innervation is sudden and complete.

In normal axolotls we have examined the mode of termination of nerves extending into muscle beyond the zones of functional

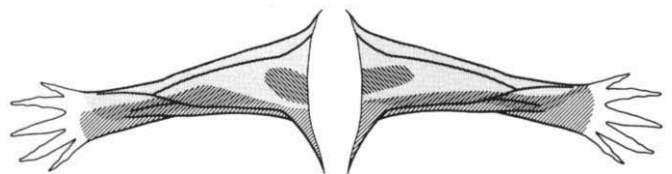


Fig. 2 Segmental innervation of muscles on the flexor surface of the hind limbs of an axolotl 9 months after breaking and crossing the major limb nerves. In this and subsequent figures the shading of nerve territories follows the convention of Fig. 1 and the experimental limb is on the left of the illustration, the untouched control limb on the right. In spite of disorganization of the nerve supply the functional innervation as revealed by intracellular junction potential recording on the operated side (left) mirrors that on the normal side (right).

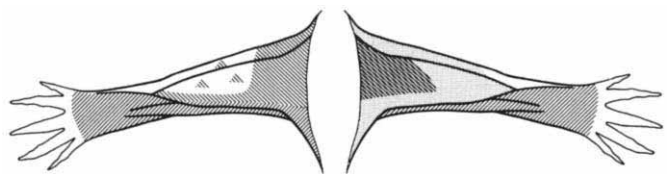


Fig. 3 Extension of the territories of muscle innervation on the flexor surface of an axolotl limb 7 d after cutting the sixteenth spinal nerve (left), as compared with the normal side (right). The territories of the fifteenth and seventeenth have expanded at the base of the limb. The triangles in the white area signify patches of muscle fibres innervated by the fifteenth nerve surrounded by non-innervated fibres.

innervation. Borders of spinal nerve territories were found by electrical stimulation and intracellular recording of junctional potentials in muscles. The corresponding nerve terminals were identified later using electron microscopy to detect neuromuscular junction made to degenerate partially by previous nerve section. Four days after cutting one spinal nerve muscle samples taken from either side of the previously defined functional border were found to contain a mixture of normal and degenerating synaptic terminals. Within the territory of the cut spinal nerve they were found in proportions of approximately three degenerating to one normal one; outside the territory there were three normals to one degenerating terminal. The normal synapses within the territory, and the degenerating one outside it, were presumably from the non-functional innervation, which therefore ends in neuromuscular junctions morphologically indistinguishable from normal synapses. This is also the case for repressed innervation of fish muscles⁹.

The normal occurrence of mutual repression of competing synaptic connexions may explain how a sharp border of functional connectivity can be formed between segmental territories. For example, if the chemospecific nature of the muscle cells changes progressively from the anterior to posterior border of the limb, the sixteenth nerve will be favoured over the seventeenth anteriorly and the seventeenth over the sixteenth posteriorly. Competition towards the centre of a field, working by mutual repression of competing synaptic terminals, could result in an abrupt changeover in the dominance of connexions. Furthermore, 3 d, which is the time taken for dormant neuromuscular synapses to be reawakened, is barely long enough for detectable ultrastructural degeneration of nerve terminals after axon section. The first result of nerve section is to silence the innervation while that from adjacent roots remains active. Therefore, in the changeover zone, the relative amount of impulse traffic carried by mutually competing nerves, rather than their structural presence, may influence the functional connectivity that they establish with a common post-synaptic muscle cell. Experiments compatible with this interpretation have been reported briefly by Aguilar *et al.*¹², and others have been done by us. Therefore the expression of intercellular recognition mechanisms, on which the formation of ordered connexions are thought to depend, can apparently be modified by the relative impulse activity of competing synapses.

There is one other published account of apparently functionless cross-innervation in salamander limbs. Grimm crossed flexor and extensor muscle nerves in the forearm of axolotls and after recovery of limb movement tested connexions by electrical stimulation. The original nerve trunks, above the point of crossing, had regained control of the muscle they originally innervated, showing that the visible crossed nerves entering the muscles lower down were mainly ineffective and that contraction depended on a few fibres recrossing to get to their right muscles¹³.

We suggest that the detailed pattern of muscle innervation in the development of these limbs depends upon widespread nerve growth and subsequent competitive interactions between nerve terminals for the control of muscles. Other develop-

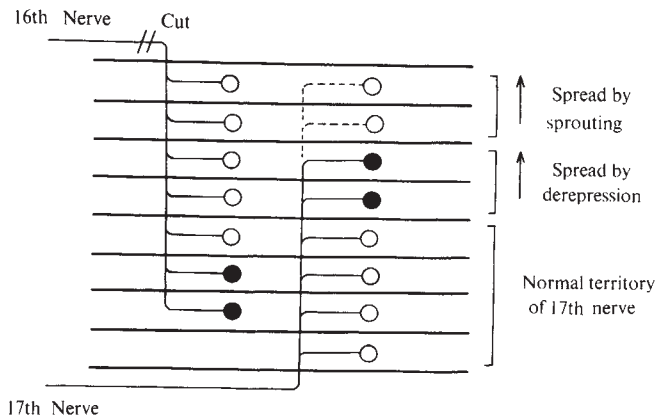


Fig. 5 Proposed mechanisms of spread of segmental innervation of muscle fibres. Repressed terminals (black) are normally present on the borders of spinal nerve territories and become functional upon silencing or degenerating out the competing innervation. This takes about 3 d. Further extension of innervation into denervated muscle is possible by sprouting of remaining nerves.

mental forces are apparently at work to direct nerve fibre growth to approximately the right place in the limb¹⁴, so that normally the competition between nerves leaves only a fringe of functionless innervation on the borders between natural muscle territories. In a regeneration or transplantation experiment such as was originally carried out by Weiss, however, much of the anatomically traceable innervation is likely to be quite out of place in the limb and therefore functionally disconnected from muscles. It is easy to see now why he became convinced that the principles of specific anatomical connectivity could not explain the restoration of function in grafted supernumerary limbs.

Both the mechanisms regulating functional nerve-muscle connexions could be important in the development and modifiability of interneuronal connexions in the central nervous system. Mutual repression of competing synapses may be required for the formation of topographically organized connexions between sets of interneurons such as are found in afferent pathways from the skin or eye. This may leave a fringe of functionless synapses just as in salamander muscle. Where the chemospecific preferences of cells are not so strong, as in densely interconnected neurones of higher levels of the nervous system the relative amount of use of a synapse may sway the recognition mechanisms in its favour, to the disadvantage of other convergent connexions. Use-dependent changes in connectivity during development, like those described in visual systems, could be attributable to a similar mechanism of mutual, functional repression of synaptic transmission, without immediate changes in ultrastructure¹⁵.

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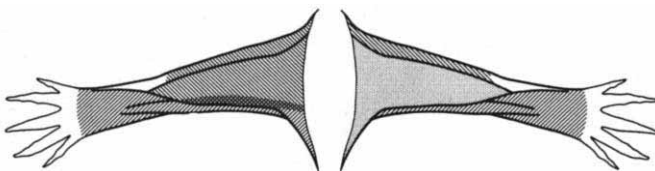


Fig. 4 Pattern of segmental innervation of flexor muscles of axolotl hind limbs 3 d after cutting the sixteenth root a second time. The animal had completely recovered from the first denervation done three months before. The whole of the territory of the sixteenth nerve on the operated side (left) is innervated by terminals from the fifteenth or seventeenth spinal nerves.

Cosmic Abundance of Boron

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The abundance of boron is of considerable interest, but is difficult to determine. Boron undergoes thermonuclear destruction in stellar interiors, and hence is not a normal product of stellar nucleosynthesis. Thus its abundance is low, and until now it has seemed possible to understand its production in nature as a consequence of cosmic ray bombardment of the interstellar medium¹. We point out here that the cosmic abundance of boron has been greatly underestimated, and that the upward revision of its abundance has important consequences in several areas of astrophysics.

In this article, we shall express all abundances relative to 10^6 atoms of Si. This requires conversion of abundances relative to 10^{12} atoms of H, using the factor 0.03175 (A. G. W. C., unpublished).

An average abundance of boron in ordinary chondrites^{2,3} is 6.2. Boron is very difficult to measure quantitatively in meteorites, but recently it has become apparent that the boron abundance is highly variable, and in particular that the abundances in carbonaceous chondrites are very much higher than those in ordinary chondrites⁴. A. G. W. C. based his 1968 table of abundances⁵ as much as possible on those in type I carbonaceous chondrites, or C1 chondrites, on the grounds that the more volatile elements are depleted relative to C1 abundances in all other types of chondrites. Thus the C1 meteorites have seemed to him to be the least fractionated source of abundances of the elements in the solar system apart from the Sun.

A spectrographic method⁶ suggests an abundance of 126 for boron in a C1 meteorite. In four colorimetric analyses of two C1 meteorites, Mills⁶ found an average abundance of boron of 5.7 parts per million, with a concentration range of 5.1 to 7.1 p.p.m., and a resulting abundance of 144. Using a fluorimetric technique, Quijano-Rico and Wanke⁷ obtained an abundance of 186 for boron in one C2 meteorite, and an abundance of 121 in three C3 and C4 meteorites. A straightforward average of these values indicates a boron abundance of about 140. But the seeming constancy of these values of the boron abundance among the carbonaceous chondrites is inconsistent with the great depletion of boron in ordinary chondrites, as we shall

discuss. This has motivated us to make a cosmochemical analysis of the condensation conditions for boron in a low pressure gas of solar composition, such as may have existed in the primitive solar nebula from which meteoritic materials condensed.

Because some of our discussion relating to the boron abundance in the Sun requires knowledge of the abundance of beryllium, we also discuss that abundance. Beryllium has an abundance much less variable in chondritic meteorites than boron, the average abundance in ordinary chondrites^{2,3} being about 0.69. The preferred data on beryllium were published by Sill and Willis⁸ who measured only one carbonaceous chondrite of type C2 and found an abundance of 0.81. This value has been adopted tentatively within the cosmochemical analysis we discuss here.

Condensation of Boron and Beryllium

Recent attempts⁹⁻¹³ at predicting the behaviour of the elements during the condensation of the solar nebula have been based upon theoretical studies of the sequence of condensation of minerals from a cooling, low-pressure gas of solar composition. Be has been ignored in these thermodynamic calculations and the only work on B to date indicates it should not have condensed above 1,700 K at a partial pressure of hydrogen in the solar nebula of 0.5 atm (ref. 9).

Grossman¹³ assumed that each condensate in the solar nebula remained in complete chemical equilibrium with the vapour, allowing a prediction of the variation of the principal element composition of the co-existing vapour with falling temperature. He solved the full equations only at temperatures greater than 1,200 K at 10^{-3} atm in that study, but we use the extrapolated vapour composition data at the lower temperatures required for B condensation, because less than 10% of the total condensable matter was left in the vapour state at 1,200 K. The presence of trace quantities of B and Be has a negligible effect on the principal element composition of the vapour.

Of forty-eight gaseous molecules containing boron for which thermodynamic data are readily available¹⁴, only HBO_2 , H_3BO_3 , NaBO_2 , and KBO_2 were found to be important in the temperature range of interest. Using a boron cosmic abundance of 140, we investigated the condensation temperatures of the crystalline phases KBO_2 , HBO_2 , H_3BO_3 , NaBH_4 , BN , NaBO_2 , TiB , MgB_2 , B_2O_3 , TiB_2 , $\text{K}_2\text{B}_6\text{O}_7$, MgB_4 , $\text{Na}_2\text{B}_4\text{O}_7$, $\text{K}_2\text{B}_6\text{O}_{10}$, $\text{B}_{10}\text{H}_{14}$, $\text{H}_4\text{B}_2\text{O}_6$, and CB_4 . Because of the relative stabilities of both gaseous and crystalline NaBO_2 and KBO_2 , the principal difficulty in determining the condensation temperature of boron arises from uncertainty in the pressures of Na and K, two of the uncondensed elements at 1,200 K. Selecting a range of four orders of magnitude in each of the partial pressures of Na and K leads to a range of B condensation temperatures between 759

and 745 K (as NaBO_2) at a total pressure of 10^{-3} . These calculations assume that the most stable gaseous compounds and the highest condensing crystalline compounds of B in a solar gas have been considered and that B does not condense in solid solution at higher temperature.

We applied the same type of calculation to Be, assuming a cosmic abundance of 0.81. $\text{Be}(\text{OH})_2$ and BeOH are the dominant gaseous species in the 1,200 to 1,800 K range. Be is almost totally condensed as BeAl_2O_4 in solid solution in spinel at 1,400 K. Again we assume that no important gaseous or crystalline species have been omitted from the consideration.

Many moderately volatile elements such as Mn, the alkalis, Cu, Au, Ga, and Ge exhibit abundance anomalies in the different classes of carbonaceous chondrites. In the C2 and C3 meteorites their concentrations, relative to Si, are all about 0.55 and 0.33, respectively, of their abundances in C1 meteorites. These relatively constant abundance depletions have been discussed in detail by Larimer^{10,15} and Anders¹⁵⁻¹⁷. They have proposed that the carbonaceous chondrites are mixtures of fine-grained matrix material which remained in condensation equilibrium with the nebula down to ~ 350 K, and chondrules and metal grains which stopped equilibrating with the nebular vapour between 1,150 K and 1,250 K at 10^{-4} atm total pressure. The results obtained by Grossman¹⁸ suggest that these temperatures should be 75 K lower. The low temperature matrix thus contains its full complement of the fractionated elements and the high temperature chondrules are devoid of them. The C1 meteorites are almost entirely matrix material and the average chondrule/matrix ratios in the C2 and C3 meteorites are in the ranges required by this model to account for the observed abundance depletions.

At 10^{-4} atm, Be would have condensed at about 1,325 K, somewhat above the maximum condensation temperatures, 1,075 to 1,175 K, of the fractionated elements. Thus, Be should not be fractionated among the carbonaceous chondrites and the abundance in the C1 meteorites must be very close to that in the C2 meteorites, 0.81. Boron, however, would have condensed at about 700 K at 10^{-4} atm and should, therefore, be classed among the volatile elements which are fractionated between the different types of carbonaceous chondrites. Indeed, abundance data⁷ indicate that the ratio of boron in C2 to that in C3 and C4 meteorites is 1.54, which is within 10% of the average ratio, 0.55/0.33, observed for the other volatile elements. This suggests strongly that the abundance of boron in C1 meteorites could be calculated from the C2, and the C3 and C4 abundances, by dividing them by 0.55 and 0.33 respectively, yielding boron abundances of 339 and 367. If the boron abundance is increased by this much over that used in the condensation calculation, the condensation temperature is increased by only about 25 K, which is unimportant for the purposes of this computation.

It is thus our opinion that the boron abundances in C1 meteorites found by Harder⁵ and by Mills⁶ are too low, and we therefore adopt a value of 350 for the cosmic abundance of boron. We emphasize that it is highly desirable that this conclusion should be checked by better analyses of boron in C1 meteorites. For beryllium we adopt the C2 abundance of 0.81 as the cosmic abundance.

Boron in the Interstellar Medium

A tentative determination of the abundance of boron in the interstellar medium was made following a rocket ultraviolet measurement (A. M. Smith and T. P. Stecher, unpublished). In this work a feature at 1,362.46 Å in the spectrum of Zeta Ophiuchi was identified as interstellar boron II. A curve-of-growth analysis indicated a ratio of boron to oxygen in the interstellar medium of 1.9×10^{-5} . An oxygen abundance of 2.15×10^7 yields a boron abundance of 418. Although one must be cautious about accepting abundance determinations based on measurements of only one line, it is interesting that this boron abundance is in essential agreement with that which we

deduced in the preceding section. It is to be hoped that this matter can be investigated further with Copernicus.

Abundances in the Sun

The situation concerning the abundance of beryllium in the Sun has been summarized recently by Hauge and Engvold¹⁸. Their abundance values have been converted to the silicon scale. The beryllium abundance is derived from the BeII resonance doublet at 3,131 Å. One of the lines is seriously blended and should not be used for abundance analyses. Beryllium abundances in the Sun were found in 1968 to be 0.47 by Grevesse¹⁹ and 0.29 by Hauge and Engvold²⁰, and in 1970 Muller obtained a still lower abundance of 0.20 (quoted in ref. 18). From this last value one would conclude that beryllium is depleted to 0.25 of its normal cosmic abundance in the material at the solar surface, although we emphasize that one must be very cautious in using abundances determined from only one spectral line.

The situation concerning boron in the Sun has also been discussed by Hauge and Engvold¹⁸. Boron has not been detected in the Sun. The strongest lines fall in inaccessible spectral regions. Grevesse¹⁹ concluded that the boron abundance is less than 20 from the absence of two BI lines at 11,661 Å. Engvold²¹ concluded that the boron abundance was less than 10 following an unsuccessful search for the absorption band spectrum of the BH molecule in sunspots. We have checked to see whether any boron molecular species should be expected under the conditions in which sunspot spectra are formed, other than those considered by Engvold²¹, and we conclude that no significant amount of boron can be hiding in some molecular form not considered by Engvold. From this it seems that boron in the surface layers of the Sun has been depleted by at least a factor of 35.

For comparison, we note that the abundance of lithium²² in C1 meteorites is 49.5, which can be compared with the solar photospheric value²³ of 0.20. Thus the solar depletion factor is about 250.

There is a clear inconsistency among the solar abundances. Lithium, beryllium, and boron are all destroyed at hydrogen-burning temperatures in the central regions of the Sun. The rate of destruction of beryllium is intermediate between the rates at which the lithium and boron isotopes are destroyed. Therefore it seems necessary to conclude either that the one line in the solar spectrum from which the beryllium abundance has been obtained is really due to some other element, or that the boron abundance in the Sun is actually an order of magnitude greater than the upper limits which have been set by the several searchers for it. Because many different lines of boron and boron compounds have been searched for in the Sun, without success, we believe that the solar beryllium line is suspect and should be re-examined.

If we accept that beryllium and boron, as well as lithium, are strongly depleted in the solar surface layers, then an unexpected amount of mixing has taken place within the solar interior some time during the history of the Sun. Using the nuclear reaction rates tabulated by Fowler, Caughlan, and Zimmerman²⁴ we conclude that the destruction of beryllium and boron will occur on a time scale of 10^7 to 10^8 yr in the deep solar interior, provided the temperature is in excess of 5×10^8 K. In a model of the Sun which had been evolved without mixing for 4.6×10^9 yr, provided by D. Ezer, 75% of the mass in the interior is at a temperature greater than 5×10^8 K. Hence almost all of the outer 25% of the solar mass has been replaced at some time by material coming from a much greater depth.

This raises some interesting questions concerning the helium isotopes in the Sun. In the evolved but unmixed model of the Sun, the ratio of ^3He to ^4He rises steadily with increasing radial distance from the centre of the Sun, reaches a maximum at a temperature of 7×10^8 K at a point at which approximately half the mass is contained within the interior radial distance, and then decreases toward the low initial value in the solar surface layers. The maximum value of the ratio is 0.17, which

is orders of magnitude greater than the ratio of the isotopes observed in the solar wind.

So mixing within the solar interior cannot have brought material from near the centre to the surface in recent times, or the ratio of the helium isotopes would be much greater in the solar wind than the observed values²⁵ which are about 4×10^{-4} . This suggests that the extensive mixing within the solar interior occurred rather early in its history, before the ^3He had had time to build up a substantial abundance following the onset of the proton-proton thermonuclear reactions. One possible mechanism which seems consistent with this requirement is the operation of Eddington-Sweet circulation currents in the Sun^{26,27}, which would be quite rapid if the initial Sun were spinning at an angular rate nearly sufficient to produce rotational instability at the equator. Such circulation currents would be shut off when the Sun developed a substantial gradient in the mean molecular weight following conversion of hydrogen to helium near the centre.

Because of these considerations we think it is dangerous to make the common assumption that the abundance of ^3He in the surface layers of the Sun represents the sum of the initial abundances of ^3He and deuterium in solar material. The abundance of ^3He may have been modified by the mixing process.

Boron Production by Cosmic Rays

One of the seemingly more successful theories of recent years has been that of Reeves, Fowler, and Hoyle¹, who found that cosmic ray bombardment of the interstellar medium could successfully account for the abundances of the light elements in the solar system. This theory, in its more recent refinements²⁸⁻³¹, seems to account quantitatively for the production of ^6Li , ^9Be , ^{10}B , and ^{11}B . It did not produce enough ^7Li to agree with the observed abundance of that isotope, but there are other possible astrophysical sources which can produce this³². The theory accounted for the older low values of the boron isotope abundances.

The great upward revision of the boron abundance requires modifications to this theory. We emphasize that the cosmic ray production theory continues to account quantitatively for the abundances of ^6Li and ^9Be . But an additional mechanism must be found which can produce the boron isotopes.

We note that Fowler, Reeves, and Silk²⁸ have argued that a large flux of low-energy cosmic rays (~ 5 to 10 MeV) could not exist in sufficient quantity to heat the interstellar medium, because this flux would have produced too much boron. The new boron abundance is so much higher than the old one that this restriction on heating of the interstellar medium by a hypothetical flux of low-energy cosmic rays is now removed. Instead, one should ask whether the hypothetical flux of low energy cosmic rays might be the necessary mechanism to produce the additional boron.

Boron-10 is produced by the reaction $^{13}\text{C}(p,\alpha)^{10}\text{B}$, which has a Q value of -4.063 MeV. Boron-11 is produced by the reaction $^{14}\text{N}(p,\alpha)^{11}\text{B}$, which has a Q of -2.922 MeV. The abundances of ^{13}C and ^{14}N in the interstellar medium seem to have the same relative values as in the solar system³³. Thus, in order to produce the solar system ratio of the boron isotope abundances, it is a necessary condition for the validity of the low-energy cosmic-ray bombardment hypothesis that the cross-section for the production of ^{10}B from ^{13}C , properly averaged over a cosmic-ray bombardment spectrum, should be higher than that for the production of ^{11}B from the proton bombardment of ^{14}N by a factor of 6.8. D. Bodansky provided unpublished cross-section data on the reaction $^{13}\text{C}(p,\alpha)^{10}\text{B}$ and J. W. Truran informs us that, according to these data, the ratio of the average cross-sections for the two reactions is much less than 6.8.

Thus we conclude that it is no longer possible, from boron abundances, to argue that the heating of the interstellar medium may not occur primarily from a flux of low-energy cosmic rays. Such a flux may contribute significantly to the production of

the boron isotopes, but we conclude that it is not the principal contributor to the production of these isotopes.

Boron Production from Supernovae

One of us³⁴ has recently prepared an extensive analysis of the structure of shock waves in supernova explosions, demonstrating that this structure may give rise to the production of the deuterium in the Galaxy through spallation of the helium contained in the outer parts of the supernova envelope. A substantial production of the boron isotopes should also occur under these circumstances. We give a brief account of the analysis here.

If a supernova explodes as a result of a nuclear detonation process in which about 1 MeV per nucleon is produced, and this supernova contains an extended atmosphere of hydrogen, essentially in a red giant configuration, then as the supernova shock wave progresses through this atmosphere, the rapidly decreasing density in the envelope will result in a strengthening of the shock wave. If the only constituent in the material traversed by the shock wave were the ions, the temperature of the ions behind the shock front would become progressively higher as the shock wave strengthens. Thus one would typically get ion temperatures of several MeV near the surface of the supernova envelope.

In fact, the energy behind the shock is shared between ions, electrons, and photons, and because of the low density most of the energy goes into the photons. The typical temperature behind the shock will be closer to one keV. But this asymptotic condition can only be approached gradually, because a very large number of photons must be created for each ion present, and these photons must then be heated.

The actual shock process starts with a heating of the ions in the shock front precursor by collision between the ions of the stationary gas and the ions moving in the shock surface. These collisions raise the ion temperature to several MeV. Energy is now fed from the ions to the electrons, which are heated to a temperature of about $m_e c^2$. Collisions between the electrons and the ions create photons by the bremsstrahlung process; most of the photons have very low energy. Collisions between the electrons and the low energy photons raise the average energy of these photons. The ion energy is thus channelled through the electrons into photons and, as the ion temperature gradually falls, the temperature of the electrons will eventually also fall as the system approaches thermal equilibrium.

The quantitative estimate indicates that it takes about 140 Compton collision periods for the initial high ion temperature to drop by about a factor of 2. During this time the collisions among the ions have brought about an approach toward a Maxwellian energy distribution; the distribution should be fairly accurately Maxwellian to about $4kT_i$, where T_i is the ion temperature, and beyond this there will be a gradual depletion of the high energy tail relative to the Maxwellian distribution. An ion temperature of about 10 MeV is sufficient to spallate most of the helium present in the medium, producing deuterium both directly and by way of the production of neutrons which are captured on the protons of the medium following the thermalization process.

The production of the boron isotopes from ^{13}C and ^{14}N has much lower spallation thresholds than that for production of deuterium from helium. Therefore we expect the production of the boron isotopes to take place at a much lower ion temperature than that at which the deuterium production takes place. This is fortunate, for otherwise the ^{10}B would be severely depleted by the neutrons that are produced in the spallation of helium. The ^{13}C and ^{14}N target nuclei may or may not have been enhanced in the outer envelope of the supernova during the preceding evolution of the star.

The situation is much more complex than in spallation processes taking place in the interstellar medium. The ion temperature remains high for a long enough period so that

multiple nuclear reactions can take place on each affected nucleus. A reaction which is of obvious potential importance is $^{10}\text{B}(\text{p},\alpha)^7\text{Be}$, which is exothermic with a Q value of 1.146 MeV. If, as we believe, this supernova production mechanism has been primarily responsible for the production of the boron isotopes in nature, then this secondary reaction may be primarily responsible for the production of ^7Li , which is formed following electron capture on ^7Be .

All of the product nuclei, ^7Be , ^{10}B , and ^{11}C , may be destroyed by further spallation reactions. Because it is possible that only a fraction of the produced material survives following the thermalization of the shock wave, because the cross-sections for destruction of the product nuclei are in general not known, and because the abundances of the target nuclei are uncertain, we do not feel in a position to predict the relative production of ^7Li , ^{10}B , and ^{11}B which results from the supernova shock process. The abundances of these product nuclei relative to the target nuclei, ^{12}C and ^{14}N , in the solar system, are of the same order as the initial deuterium abundance in the primitive solar nebula relative to the solar helium abundance. Thus it is not quantitatively implausible that the abundances of deuterium, ^7Li , ^{10}B , and ^{11}B , should all be produced in their solar system proportions in these supernova shock waves.

The review article on the meteoritic abundances of boron by Baedeker⁴ first drew our attention to the necessity for this upward revision in the cosmic abundance of boron.

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Emission Mechanism in Pulsars

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In a new model of the emission from pulsars based on the relativistic beaming effect, the source of the radiation is the circular motion of high energy electrons round magnetic field lines. The radio emission at the gyrofrequency is from coherent bunches of electrons, and the optical and X-ray emission is the incoherent synchrotron radiation from the same electrons.

ALTHOUGH there is as yet no agreement on the location of the source of the pulsed radiation within the magnetosphere of a pulsar, there is now considerable evidence that it is located close to the velocity of light circle, so that the corotation of the

magnetosphere moves the source with a velocity of the order of 0.8 to 0.9 c . The pulses are then formed by the process of relativistic beam compression¹⁻⁵, which allows the radiation process to be isotropic within the local frame of reference within the magnetosphere, giving a radiation beam solely due to the geometrical movement of the whole source.

Here I discuss the nature of the radiating source which the observations would require if the beaming effect is indeed primarily due to relativistic beam compression. I shall show that a simple and self-consistent model can explain the whole of the electromagnetic radiation from the Crab Nebula pulsar, including some of the more detailed characteristics of polarization and variability. The lack of optical pulses from other pulsars is also explained simply, as indeed it is on all other models so far put forward.

Characteristics of the Emitter

Although the Crab Nebula pulsar is the only pulsar known to radiate outside the radio spectrum, there is no reason to

suppose that its radio emission differs basically from that of other pulsars, for which the radio emission has been studied in greater detail. One may therefore list the following features of pulsar emission which must be explained by any comprehensive model: (i) the integrated pulse envelope; (ii) the individual sub-pulses; (iii) the microstructure, appearing as sharp pulsations within a single sub-pulse; (iv) the polarization structure in the integrated pulse envelope; (v) the polarization of the sub-pulses; (vi) the variability of the radio pulses; (vii) the lack of variability in the optical pulses; (viii) the rapid decrease of the radio spectrum with frequency; (ix) the flatness of the optical spectrum and the decreases both in the infrared and the ultraviolet; (x) the extension of the spectrum to X-ray and gamma-ray frequencies; (xi) the small variation of the integrated pulse envelope over the whole electro-magnetic spectrum, that is, over more than fifty octaves; (xii) the evidence of a narrow-band character in the microstructure of pulses; and (xiii) the extremely high volume emissivity both in the radio and in the optical and X-ray spectrum.

I shall discuss these properties in the light of a proposed model which seems to be capable of accommodating all of them.

Model Emission Mechanism

I suggest that the radio emission is from bunches of electrons gyrating round the magnetic field within a small range of longitude defined by a particular field configuration. The radio radiation is coherent at the gyrofrequency and is narrow band. The same electrons, which have relativistic energies, radiate broad-band synchrotron radiation at high harmonic frequencies, that is, in the optical and X-ray spectrum. This harmonic radiation is not coherent.

As the radio emission is narrow band, the observed wide radio spectrum must correspond to a distribution in resonant frequency of the narrow-band sources. This distribution must also be a spatial distribution, so that different parts of the source have different resonant frequencies. The optical and X-ray spectrum will then be determined largely by the corresponding distribution of synchrotron critical frequency, which can be regarded as a combination of gyrofrequency and particle energy.

This model is distinguishable from most others in three ways which I shall discuss in the following sections: (i) the radio emission is narrow band, (ii) the pulse width is related to the relativistic movement of the source rather than to a radiated polar diagram; and (iii) optical and X-ray emission can occur from the same particles as those which are emitting radio waves.

Spectra of Pulsars

Published radio spectra⁶ refer to the total energy in the whole pulse, averaged over many pulses. These spectra show a monotonic steep fall with frequency above about 300 MHz for all pulsars, with spectral indices generally between -2 and -3 . Below 300 MHz many spectra become flatter, and some show a reverse slope below 100 MHz.

These spectra have generally been interpreted as emission from a broad-band source. There is, however, considerable evidence that the basic radio emission process is in fact narrow band, so that the broad spectrum represents the sum of emission from an ensemble of narrow-band sources.

The principal evidence is as follows:

(i) The integrated profile for many pulsars is dependent on radio frequency. If this profile represents a spatial distribution of sources, then the dependence must be interpreted either as large differences in the spectra of moderately broad-band sources, or more probably as a relation between frequency and position of narrow-band sources. In pulsars such as PSR 1133+16, some parts of the profile have a very steep slope, corresponding to a spectral index of -5 . An even steeper slope has already been reported for the "precursor" component of the Crab Nebula pulsar⁷. These steep slopes suggest a high

frequency cutoff, more consistent with an end to the range of narrow-band sources than to a sharp spectral feature in the radiation from a single broad-band source.

(ii) The "microstructure" of pulses from PSR 0950+08, as observed at 408 MHz, has a bandwidth of only about 20 kHz (ref. 8); this is not far from the inverse of the microstructure duration, suggesting that the sources of individual pulses are basically monochromatic but with a rapidly changing excitation.

By contrast, individual sub-pulses usually have large bandwidths⁹. The narrowest known frequency structure here is in the giant pulses from the Crab Nebula pulsar, which are often unrelated at frequencies as close as 74 and 111 MHz (ref. 10). The simplest interpretation seems to be that the sub-pulses contain narrow-band emission excited simultaneously, or nearly simultaneously, from sources covering a wide range of frequency.

Pulse Width and the Polar Diagram

It is part of the hypothesis of relativistic beam formation, which is adopted here, that no part of the time structure of the pulses need be identified with a beam formed in the emission process itself. If there were any large directivity in the emission process, then the sub-pulses would be correspondingly reduced in width. Because the directivity of any emitter can be expected to vary with wavelength, the sub-pulse width would then vary with radio wavelength, which is contrary to the available evidence. I therefore assume that the emission polar diagram is wide; it may even possibly be isotropic. Furthermore, a single sub-pulse presents a cut through this wide polar diagram covering about 180° , the cut being compressed into a pulse by the relativistic effect.

The clearest distinguishing feature of the sub-pulses is that they are, as individuals, typically 100% polarized¹¹. The changes of polarization during a sub-pulse can therefore be represented by the movement of a point on the surface of a Poincaré sphere, which is a surface on which the two hands of circular polarization are represented by points at the poles, linear polarizations with various position angles are represented along the equator, and elliptical polarizations appear at intermediate latitudes depending on their ellipticities.

The observed changes of polarization within a sub-pulse correspond to a smooth movement over the Poincaré sphere of up to half of one circumference, at an arbitrary position angle. For example, the hand of polarization often changes once, but never twice, during a single sub-pulse; again the position angle often changes through a right angle, but seldom more unless there is a large degree of circular polarization.

Allowing for relativistic beam compression, this result must be interpreted as an emission pattern which includes all possible elliptical polarizations, from plane to circular, varying smoothly over a wide range of angles, probably reaching 180° . It has already been remarked¹ that this is a good description of the polar diagram of cyclotron radiation, and that no such pattern can be produced by curvature radiation from particles streaming along field lines.

Synchrotron Emission

The electrons gyrating in bunches round the field lines may have high relativistic energies and they will therefore radiate synchrotron radiation. The radiation will be incoherent for wavelengths smaller than the bunch size, so that the radiated power is simply proportional to the number of electrons. The large variations in the radio pulses are then seen to be variations in the bunching process, which leaves the optical emission constant, in accordance with observation¹².

Synchrotron radiation is beamed in the direction of particle motion. A single electron with energy γ gives a beam width of $1/\gamma$ at frequencies close to the critical frequency; the beam is fully polarized, generally elliptically but plane polarized at the maximum. The observed optical polarization of the Crab Nebula pulsar is linear, approximately 10%, and varying through the pulse. This is evidently explicable as the sum of

synchrotron radiation from an ensemble of sources, spread in longitude. Such an ensemble would average out the circularly polarized components, and reduce the plane polarization.

The synchrotron spectrum of a single electron is expected to be flat in the region of the critical frequency, falling exponentially at higher frequencies, and varying as $\nu^{1/3}$ at lower frequencies. The ensemble of sources may have a spread in particle energy, and hence of critical frequency; there may also be a low frequency cutoff due to self-absorption.

The observed spectrum of the Crab Nebula pulsar is flat in the optical region, which may tentatively be identified with the flat region just below the critical synchrotron frequency.

Numerical Values

The radio volume emissivity, averaged over a sub-pulse, has previously been estimated on the basis that the emitting volume occupies about 0.05 of a sphere which extends out to the velocity of light circle, that is, $0.05(4/3)\pi(c/\Omega)^3$. There is no real basis for taking any other volume, although it is to be expected that the actual volume may be considerably smaller. The recent observations^{13,14} of intense sub-pulses now suggest an increase of volume emissivity of a factor of 40 for PSR 0950+08, so that typical values for volume emissivity at radio wavelengths are 10^8 W m⁻³ for PSR 0521+22 and 10^4 W m⁻³ for PSR 0329+54. Both these refer to a bandwidth of 100 MHz at a centre frequency of 100 MHz. The emissivity is assumed to change with the short time scale observed by Hankin.

The power P_c radiated by a single electron at its fundamental cyclotron frequency ν , whatever its energy, is

$$P_c = (8\pi^2/3)(e^2 v^2/c^2)$$

Typically I consider radiation observed at 200 MHz, which is radiated at about 50 MHz and Doppler shifted due to the motion of the whole source. Then

$$P_c = 5 \times 10^{-20} \text{ W}$$

Suppose the volume density of radiating electrons is N m⁻³ and that all electrons are bunched over a volume of about 1 m³, giving full coherence at the typical frequency of 50 MHz. Then the volume emissivity is $5 \times 10^{-20} N^2$ W m⁻³, or somewhat less depending on the precise configuration of the bunch. So for PSR 0532, $N = 5 \times 10^{13}$ m⁻³, and for PSR 0329, $N = 5 \times 10^{11}$ m⁻³.

The cyclotron frequency ν_H is determined by the field H (gauss) and the electron energy $\gamma m_0 c^2$, so that $\nu_H = 2.8 H/\gamma$ MHz. For the outer part of the magnetosphere of PSR 0532 the field is believed to be 10^5 to 10^6 gauss, corresponding to a surface field of 10^{12} gauss. I deduce that $\gamma = 5 \times 10^3$ to 5×10^4 . For other pulsars the field is expected to be less, falling as the inverse cube of the period if the surface field is the same. We must therefore suppose that γ also decreases with increasing period, keeping the cyclotron frequency within the radio frequency band. I return to this point later.

A single, gyrating, highly relativistic electron radiates a total power

$$P_T = (8\pi^2/3)(e^2 v^2/c)\gamma^4$$

Most of this power is emitted at frequencies in the region of the critical frequency $\gamma^3 \nu_H$. The bunches of electrons which emit strongly enhanced radiation at the fundamental frequency ν_H will emit incoherently at these high harmonics, so that the volume emissivity will be NP_T . Using the adopted values for ν_H and γ , the critical frequency for PSR 0532 is between 10^{17} and 10^{20} Hz, that is, in the ultraviolet to X-ray region. Below the critical frequency the spectrum is expected to be flat, ultimately falling as $\nu^{1/3}$ at lower frequencies (unless there is an additional effect of self-absorption). The total power emitted depends on γ^4 , and must therefore be uncertain. But with $M = 5 \times 10^{13}$ m⁻³ the observed volume emissivity of 3×10^{12} W

m⁻³, mostly in the ultraviolet and X-ray region, would be obtained for $\gamma = 3 \times 10^4$, within the range of the expected values.

This simple analysis suggests that the coherent radio and the incoherent optical and X-radiation could be emitted by the same electrons. This is the chief conclusion of the analysis.

Implications

The analysis has so far proceeded from the observed characteristics of the radiation, without regard to any model of the pulsar magnetosphere or to the expected behaviour of charged particles within the magnetosphere. I now discuss the chief problems in relating the radiation model to our present understanding of the magnetosphere.

(i) Is there a preferred location of the emitter? The stability of the pulse periodicity implies that the source must lie on certain preferred lines of force. F. D. Kahn has suggested (private communication), and some observations confirm¹¹, that the lines of force are those which point forwards of the radius vector at the velocity of light cylinder. These occupy a suitably restricted longitude range, and they are lines on which the particles are expected to attain high energies.

(ii) Is cyclotron motion reasonable? As a first approximation charged particles move along the lines of force, with very small pitch angles. This cannot continue indefinitely along the lines which point forwards at the velocity of light cylinder; either the particles must break away or the pitch angle must increase. There seems to be a possibility that the pitch angle will approach 90° at a particular point along such lines of force.

(iii) Coherence and bunching. An instability leading to bunching has been described by Goldreich and Keeley¹⁵ who also discuss the conditions for coherent synchrotron radiation from the bunches. Their analysis refers to electrons constrained to move in a ring, all with the same energy. In the pulsar magnetosphere there must of course also be an energy supply, presumably in the form of an electron stream.

(iv) Particle lifetime. In the Crab Nebula pulsar the particle energy density deduced from the radio emission can only sustain the optical and X-ray emission for about 10^{-5} s. Although it seems possible for fresh particles to enter the emitting region at a sufficient rate, it is also possible that the incoherent radiation is produced by lower energy particles over a larger volume, with a correspondingly longer lifetime.

(v) Long-period pulsars. No obvious dependence of radio spectrum on period P has yet been found, so that the theory must allow for coherent cyclotron radiation at frequencies higher than 50 MHz for all known pulsars. Because for a dipole field the strength varies as r^{-3} , the field at the emitting region is expected to fall as P^{-3} , so that it is 10^6 smaller for the long period pulsars than for the Crab Nebula. In this model of the Crab Nebula pulsar I found $\gamma \approx 3 \times 10^4$, so there seems no possibility of obtaining sufficiently high cyclotron frequencies in all pulsars unless either the frequency is increased by Doppler shift due to velocity along the field lines, or the field strength is increased. Doppler-shifted cyclotron radiation would not show the correct polarization properties, as it emphasizes only one hand of circular polarization. I must therefore suggest that the field strength in the long period pulsars is greater than in the Crab Nebula pulsar by a factor of 10^2 or 10^3 . This may account for the relative lack of long-period pulsars, for only those with unusually large fields would in fact radiate at radio frequencies.

The simple calculations here show that the same electrons giving rise to the coherent radiation at radio frequencies could account for the radiation at optical and higher frequencies. The radio frequency radiation is narrow band, so that a full theory must include an ensemble of sources over a range of fundamental frequencies. This range must correspondingly widen the optical and X-ray spectrum, which need not show the exponential fall above a single critical frequency which is typical of synchrotron radiation.

Further elaboration of this theory could account for the variations in the integrated pulse profile, from low radio frequencies to gamma rays, in terms of small variations of field and electron energy in different parts of the source.

The absence of optical radiation in pulsars other than the Crab Nebula pulsar is understandable as a combined effect of lower electron energy and lower magnetic field. The comparatively small population of pulsars with long periods may possibly be those with the strongest magnetic fields.

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Intense Vibrational Absorption Spectra of Trapped Triatomic Radicals

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Intense spectra of triatomic radicals have been produced by means of a surface reaction of hydrogen atoms, formed by a microwave discharge in flowing gas, with diatomic oxides. Failure of this method to produce detectable formyl radicals is attributed to existence of an activation energy greatly exceeding 200 cm⁻¹ for reaction of H with CO. New NH and ND fundamental vibrations of HNO and DNO, at 2,719 and 2,043 cm⁻¹, are now consistent with the known large bond length. The weak absorption due to the bending mode of DO₂ has at last been located at 1,085.3 cm⁻¹, enabling a vibrational assignment for hydroperoxyl radical to be completed.

ALTHOUGH much quantitative structural information has been obtained on the free radicals formed by reacting hydrogen atoms with CO, NO or O₂ to form formyl, nitrosyl hydride or hydroperoxyl radicals, certain peculiarities remain. Missing vibrational fundamentals, anomalous bond-length-wavenumber correlations and activation energies for the formation reaction are some of these properties. I have extended the electric discharge method of Dewar¹, as developed by Geib and Harteck^{2,3}, to produce very intense vibrational absorption spectra which have provided information on these outstanding problems.

The basic apparatus is shown in Fig. 1. Gases from a standard high-vacuum manifold passed to 'Cryotip' refrigerators operated at 17K, mounted in 'Unicam SP100G' or 'Perkin-Elmer 225' spectrometers. The quarter-wave cavity was fed with 75–125 W

power at 2.45 GHz from a 'Microtron' generator to maintain a stable discharge in gas at 20–30 N m⁻². Both inlet systems operated with comparable efficiency. Hydrogen or deuterium (mol fraction 1–4% in argon) was discharged, whereas the oxide (CO, NO or O₂, 5–20% in Ar) was not.

When H atoms were reacted with molecular oxygen, very intense absorption at 1,389.0, 1,101.0 and another fairly intense absorption at 3,410.5 cm⁻¹ were observed (Fig. 2A). When D atoms were used instead, intense absorptions appeared at 1,020.2 and 2,529.7 cm⁻¹ (Fig. 2B). All these wavenumbers agree conclusively with those measured previously in photolysis experiments^{4–6} in which hydroperoxyl product absorptions were much less intense. To complete the assignment, careful scrutiny of several minor absorptions was necessary. Characteristic absorptions of H₂O, H₂O₂, D₂O, D₂O₂ and O₃ were readily recognized. I believe the ozone observed to be derived from reaction of oxygen atoms, produced by bombardment of the silica tube in the discharge region by energetic argon ions, with oxygen molecules. (Correspondingly CO₂ and NO₂ were observed in experiments with CO and NO.) Weak absorptions at 830 and 760 cm⁻¹ may be attributed to D₂O₄⁷, presumably formed by combination of DO₂ radicals. All these absorptions were of variable intensity relative to the 1,020 cm⁻¹ line of DO₂ in various experiments, whereas the 1,085.3 cm⁻¹ line had an intensity ratio constant within experimental error. Also notable is a linewidth phenomenon. For all three fundamentals of HO₂ and the other two of DO₂, widths at half maximum intensity were always significantly greater than those of other species detected in the same spectral scans, such as H₂O, D₂O, O₃. The 1,085.3 cm⁻¹ absorption shares this greater linewidth property. Thus both intensity ratios and linewidths support assignment of this line to DO₂.

This assignment can terminate a controversy between earlier predictions of the order of fundamentals, which generally placed ν_3 (O–O stretching) at greater wavenumbers than ν_2 (bending) and previous incomplete data from photochemical experiments⁴. The only reasonable assignment now consistent with all experimental data places ν_2 of HO₂ above ν_3 . The intensity differences, small between ν_2 and ν_3 of HO₂ but large for DO₂, are similar to those for HCN and DCN vapours.

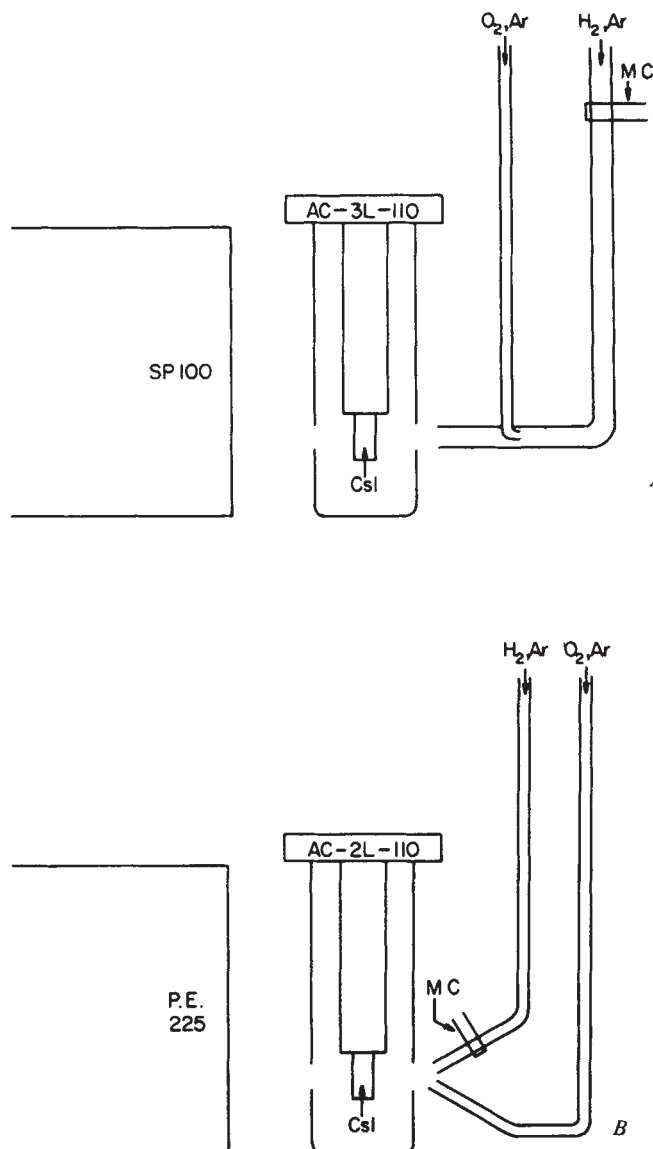


Fig. 1 Diagrammatic representation of inlet systems used to obtain intense spectra. MC, microwave cavity.

When H atoms were reacted with nitric oxide, no absorption in the 3,200–3,600 cm^{-1} was found to obey the intensity criterion, a consistent absorbance ratio with the characteristic 1,564 cm^{-1} feature of HNO, in experiments with varied conditions. A line

at 2,719 cm^{-1} did, however, obey this criterion, and one correspondingly at 2,043 cm^{-1} for DNO, as shown in Fig. 3. These lines can be attributed to no other species known to be present, such as nitrogen oxides, nitrous acid, etc. The isotope wavenumber shift is as expected. As these NH and ND fundamentals are considerably weaker than the NO fundamentals, the intense spectra made possible by the flow discharge method were invaluable for these assignments. Furthermore the isolation of HNO or DNO molecules, surrounded by only argon atoms in most sites, prevents intermolecular interactions which could lead to wavenumber shifts. When HNO is produced on photolysis of methyl nitrite or nitromethane (unpublished results) for instance, the carbonyl fundamental of cage partner H_2CO is significantly displaced from the wavenumber for unambiguous isolation in other experiments; to expect a corresponding perturbation of HNO absorption wavenumber is entirely reasonable.

When H or D atoms were reacted with carbon monoxide under similar conditions, no trace of HCO or DCO was obtained in these flow discharge experiments, but minor absorptions due to impurities, as well as major absorption of CO, were easily detected.

The spectra in Figs. 2 and 3 make clear that this method can make possible relatively concentrated samples of matrix-isolated radicals. Consideration of gas pressure, both in the reaction zone and downstream, and available time, between mixing of reagents and freezing on the Csl substrate, precludes the reaction of, for example, H with O_2 in the gas phase; known specific velocities from gas kinetic studies and the requirement of a third body (M), $\text{H} + \text{O}_2 + \text{M} \rightarrow \text{HO}_2 + \text{M}$, mean that such a homogeneous reaction is impossible with either reactor in Fig. 1. Therefore the synthetic reactions must occur at or just below the surface layers of the deposit as it builds up while incident gases freeze thereon. The efficiency of production of radicals to produce intense spectra in short deposition periods (2–4 h) with small amounts of reactants means that mobility on the freezing surface must be possible, but also the relative scarcity of radical dimers (such as D_2O_4) indicates only a limited mobility. Consistent with these results is the hypothesis that H or D atoms are very mobile species, even when having only 200 cm^{-1} of kinetic energy (thermal energy for 300 K), so that they can diffuse across and into the argon surface layers for some brief period. Of course the surface layers at time of formation cannot be considered to be representative of the bulk of the sample. Proof that reaction occurs heterogeneously is provided in other experiments in which H or D atoms were reacted with NO_2 ; observed products included *s-cis* and *s-trans* rotamers of HONO but no trace of OH radicals. In the gas phase, the rapid bimolecular reaction⁸ $\text{H} + \text{ONO} \rightarrow \text{OH} + \text{NO}$ is known to be much more efficient than the termolecular combination reaction $\text{H} + \text{ONO} + \text{M} \rightarrow \text{HONO} + \text{M}$. But even

Fig. 2 Infrared absorption of products of reaction of (A) H and (B) D atoms with molecular oxygen. Arrows mark features attributed to HO_2 and DO_2 .

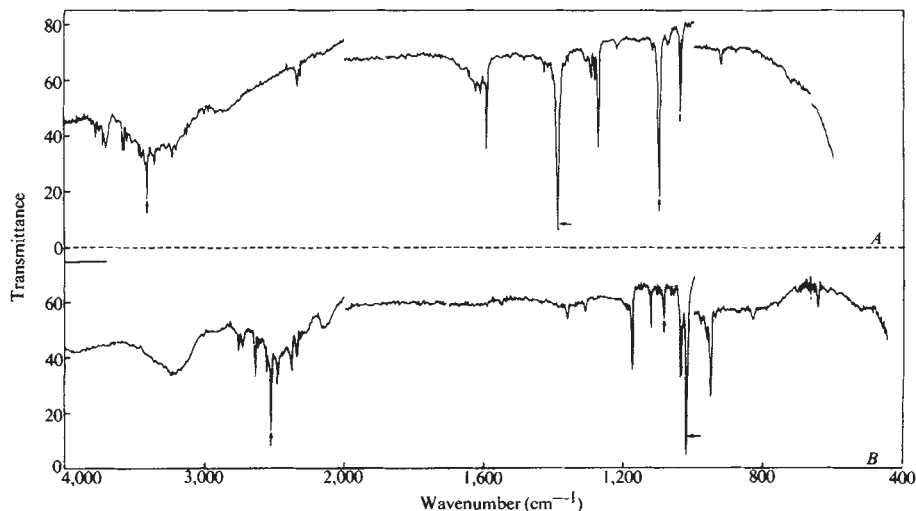
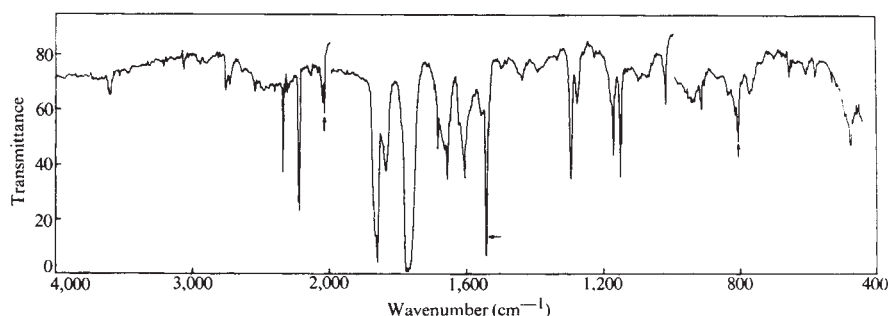


Fig. 3 Infrared absorption of products of reaction of D atoms with nitric oxide. Arrows mark features attributed to DNO.



H or D atoms can be trapped in argon; the absorption at 643 cm^{-1} in Fig. 2B is probably due to trapped D⁹. No absorption at 905 cm^{-1} is seen in Fig. 2A, but the scanning procedure used may have precluded observation¹⁰.

Lack of formyl radicals in these experiments is positive proof of an activation energy exceeding 200 cm^{-1} (2.4 kJ mol^{-1}) for the synthesis reaction. Earlier evidence¹¹ for such an activation requirement was ill-founded⁴. When hydrogen iodide was photolysed ($39,413\text{ cm}^{-1}$) as a source of hydrogen atoms⁴, $14,800\text{ cm}^{-1}$ of excess energy remained after $24,600\text{ cm}^{-1}$ was used in dissociating HI into ground-state atoms^{12,13}; most of this excess energy must, by conservation of momentum, be left to the hydrogen atom as kinetic energy. The energy of activation of self-diffusion of metals is often estimated¹⁴ to be two-thirds the energy of sublimation. If motion through the argon lattice required such an energy, 430 cm^{-1} , per step, and if almost all the excess energy available from that photodissociation had to be dissipated in this manner, H or D atoms could travel many steps from the site of production. Alternatively, a number of such translational steps could still occur while leaving the diffusing atom considerable energy for reaction initiation purposes. Radical concentrations attained in photochemical experiments suggest that the average transfer of "hot" hydrogen atoms consists of only a short motion. On the other hand, the quantity 430 cm^{-1} may be an underestimate, as infrared radiation at 905 cm^{-1} seems far more effective in causing loss of hydrogen atoms, presumably eventually by recombination, than at 644 cm^{-1} (for D atoms)⁹. In any case, we conclude that the flow discharge method is effective in producing radicals by synthesis only when at most a very small activation energy is required.

The vibrational and structural parameters of all three radicals are collected in the table for purposes of comparison. The structure of the formyl radical, previously reconsidered because of the vibrational spectra of trapped radicals⁴ but calculated purely from gas-phase rotational constants¹⁵, has now been accepted¹⁶. An alternative procedure¹⁷, based on minimization of error in calculated vibrational wavenumbers, lacks physical justification. For nitrosyl hydride, previous assignments of the HN stretching fundamental, $3,300\text{--}3,600\text{ cm}^{-1}$, were severely inconsistent with the large known bondlength and small dissociation energy (upper limit of $17,000\text{ cm}^{-1}$), according to Bernstein's correlations¹⁸. This discrepancy has now been resolved satisfactorily on the basis of the new assignments.

Table 1 Structures and Fundamental Vibrations of Triatomic Radicals

	HCO	DCO	HNO	DNO	HOO	DOO
$r_0(\text{X-H})$, pm	114.5*		106.28†		98.5	
$r_0(\text{X-O})$, pm	117.65		121.16		136	
angle, degrees	123.3		108.58		106	
ν_1 , cm^{-1}	2,482	1,926‡	2,719	2,043	3,410.5	2,529.7
ν_2 , cm^{-1}	1,088	850	1,122	811.7	1,389.0	1,085.3
ν_3 , cm^{-1}	1,861	1,803	1,564	1,547.3	1,101.0	1,020.2

* Results from ref. 3.

† Results from ref. 14.

‡ Results from ref. 10.

The structure of HO₂ has been newly estimated on the basis of the definite vibrational assignment; the OH distance is derived from Bernstein's correlation¹⁸, the OO distance from Orville-Thomas's correlation¹⁹, whereas the interbond angle is based on theoretical results but is consistent with rough rotational constants estimated for HO₂²⁰. A complete normal coordinate analysis will be published elsewhere.

Finally, the alleged absorption of trapped hydroxyl radicals²¹ ($3,452.3$ and $3,428.2\text{ cm}^{-1}$ for OH, or $2,543.2$ and $2,526.2\text{ cm}^{-1}$ for OD) is inconsistent with both earlier direct observation²² (at $3,574$ or $2,635\text{ cm}^{-1}$) and combination differences (of $3,573$ for $2,634\text{ cm}^{-1}$) in fluorescence spectra²³ of radicals trapped in neon.

Absorption spectra in the latter experiments are closely related to those of OH or OD in the gas phase, so there can be no doubt of the identity of the carrier. Purported spectra²⁰ of hydroxyl radicals are almost certainly due to hydroxyl anions, OH⁻ or OD⁻. The method of production, vacuum ultraviolet photolysis during deposition, is known to lead to anions in other cases; for example, C₂⁻ has been obtained from HCCH in this manner²⁴.

I am indebted to the late Dr K. B. Harvey for acquainting me with the problems of these radicals. Some experiments in Cambridge University Department of Physical Chemistry were conducted during tenure of a Research Fellowship of Emmanuel College. Research support at Memorial University was provided by the National Research Council of Canada and the Research Corporation (New York).

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LETTERS TO NATURE

PHYSICAL SCIENCES

Strontium Isotopes and Rare Earth Elements in Basalts from the Heimaey and Surtsey Volcanic Eruptions

THE Vestmannaeyjar archipelago has been the site of two recent volcanic eruptions (Fig. 1). The first was the Surtsey eruption from November 1963 to June 1967 and the second is the Heimaey eruption which began on January 23, 1973, and is still continuing^{1,2}. The first products of the 1973 Heimaey eruption are close to mugearite in composition. A gradational change in composition from mugearite to hawaiiite was recorded during the first week of eruption². Reaction between the mugearite magma and gabbroic xenoliths to form kaersutite and pargasitic hornblende² suggests fairly high f_{H_2O} in the magma.

The Vestmannaeyjar are almost exclusively made up of basalt lavas and tuffs of mild alkaline character, like the Surtsey lavas and the Helgafell lava². Basalts from the Vestmannaeyjar, together with basalts from the Snaefellsnes area of western Iceland (Fig. 1), are the only examples of postglacial alkali olivine basalts in Iceland³—most postglacial basalts are tholeiites and olivine tholeiites. Postglacial transitional alkali basalts³, however, occur in the Katla and Eldgja-region in southern Iceland. The areal distribution of the postglacial petrological provinces in Iceland³ is shown in Fig. 1.

Here we compare the $^{87}\text{Sr}/^{86}\text{Sr}$ ratios in lava samples erupted in 1964 and 1966 in Surtsey with those in samples of both the present, and earlier, eruptions on Heimaey, in order to see if the ratios of their source regions differ in space and time or with chemical composition of lavas. The samples of older volcanic products on Heimaey include a Recent lava in Helgafell (the mountain which was cut by the 1973 fissure), a subglacial volcanic plug in Háin, Heimaey, and a subglacial volcanic plug in Dalfjall, Heimaey (Table 1). In addition to the Sr isotope measurements, measurements of rare earth elements in three samples are reported.

Table 1 Samples Analysed

Sample	Lithology	Time of eruption
VE-112	Mugearite tephra	January 23, 1973
VE-111	Mugearite lava	January 23, 1973
I-42	Alkali-olivine basalt	1966
I-43	Alkali-olivine basalt	1964
VE-67	Porphyritic alkali-olivine basalt	5,500 yr BP
VE-72	Alkali-olivine basalt	Subglacial
VE-93	Hawaiiite plug	Subglacial

The procedures for the chemical separation of Sr and rare earth elements (REEs) will be described in detail elsewhere. Measurements of the Eimer and Amend SrCO_3 standard over the past eight months range from 0.70807 ± 6 to 0.70818 ± 4 with a mean value for eight samples of 0.70815 ± 4 (R. J. P. and R.K.O'N., unpublished data).

$^{87}\text{Sr}/^{86}\text{Sr}$ ratios have been determined precisely for two samples from the first day of the present Heimaey eruption, VE-111 and VE-112 (Tables 1 and 2). These two measurements, 0.70329 ± 7 and 0.70330 ± 6 respectively, are in excellent agreement. They may be compared with measurements for samples

Table 2 Strontium Isotope Results

Sample	Locality	$^{87}\text{Sr}/^{86}\text{Sr}^*$
VE-111 (lava)	Heimaey eruption (January 23, 1973)	0.70330 ± 6
VE-112 (tephra)	Heimaey eruption (January 23, 1973)	0.70329 ± 7
I-42 (lava)	Surtsey eruption (1966)	0.70334 ± 4
I-43 (lava)	Surtsey eruption (1964)	0.70330 ± 6
VE-67 (lava)	Helgafell, Heimaey	0.70325 ± 6
VE-72 (volcanic plug)	Háin, Heimaey	0.70323 ± 6
VE-93 (volcanic plug)	Dalfjall, Heimaey	0.70330 ± 5

* Normalized to $^{86}\text{Sr}/^{88}\text{Sr}=0.1194$, see text.

I-42 and I-43 from Surtsey and earlier volcanic products on Heimaey (VE-67, 72 and 93) (Table 2). There are no statistical differences between the Sr isotope ratios, and it is reasonable to assume that the isotopic composition of the source regions of these magmas has remained constant through late glacial and postglacial times.

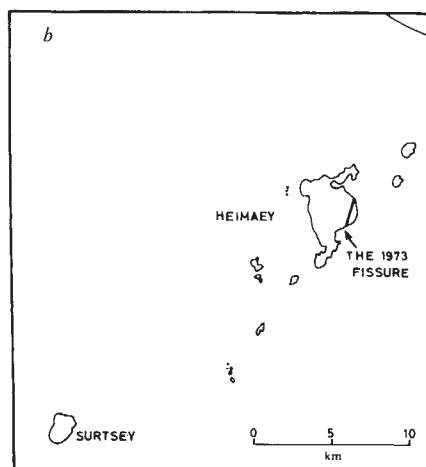
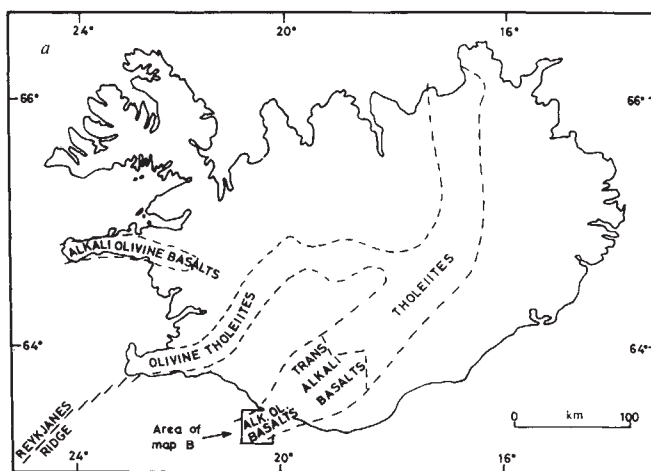


Fig. 1 a, Map showing the neovolcanic zones of Iceland, and the areal distribution of petrological basalt types (after Jakobsson³). b, Sketch map of the Vestmannaeyjar archipelago.

Table 3 Rare Earth Element Concentrations (p.p.m.)

Element	VE-112	VE-67	I-42
Ce	80.58	35.66	22.09
Nd	45.92	22.48	15.27
Sm	11.51	5.88	4.21
Eu	3.24	1.87	1.42
Gd	10.70	5.92	4.64
Dy	10.56	5.55	4.36
Er	6.05	3.21	2.32
Yb	5.39	2.70	1.80

Eight rare earth elements have been measured in each of three rocks, VE-112 and 67, and I-42 (Table 3, Fig. 2). The chondrite-normalized REE distribution pattern in each of these rocks is fractionated with the light REEs showing varying degrees of enrichment by comparison with the heavy REEs. These patterns are broadly similar to those of alkali olivine basalts from other oceanic areas⁴, but contrast with those of olivine tholeiite and tholeiite from the mainland (ref. 7 and R. K. O'N. and K. Grönvold, unpublished) of Iceland which do not show marked light REE enrichment and have overall lower abundances. These latter basalts do not on the whole show the characteristic light REE depletion of the dredged ocean ridge tholeiites⁷.

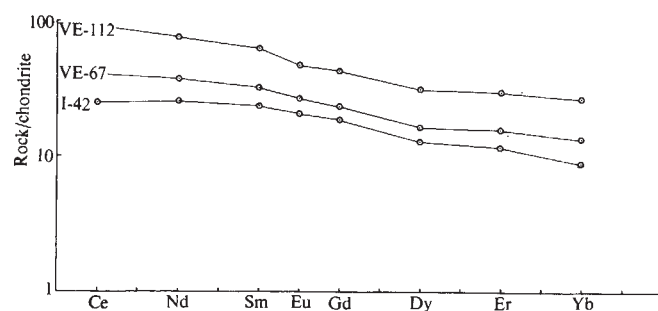


Fig. 2 Chondrite normalized rare earth distributions for a sample of the present eruption on Heimaey (VE-112), a lava sample from a 1966 eruption of Surtsey (I-42), and a sample from Helgafell, Heimaey (VE-67).

The $^{87}\text{Sr}/^{86}\text{Sr}$ ratios of the samples analysed here are statistically indistinguishable from the mean of 0.70328 for ten postglacial Icelandic basalts ranging in composition from olivine tholeiite to alkali-olivine basalt (R. K. O'N. and K. Grönvold, unpublished). It may, therefore, be reasonable to assume a common (upper mantle) source region for these basalts which is essentially chemically homogeneous. This would place important constraints on the interpretation of the trace element data.

Recently one of us³ has suggested, on the basis of seismic determinations of the depth⁶ to layer 4, that the alkali basalts in Iceland are generated at greater depths and consequently higher pressures than the tholeiites and the olivine tholeiites, and may therefore represent different degrees of melting of similar material. If the degree of partial melting in the source region has essentially controlled the trace element composition of the various basalt types, then semiquantitative estimates of the relative amounts of melting can be made on the basis of K contents alone. If the olivine tholeiites are generated by ~30% partial melting then the alkali basalts correspond to a factor of five to eight times less melting, that is, 4 to 6% partial melting. The productivity per unit area on surface in the post-glacial alkali regions is about five to six times less than in the tholeiite regions. If these estimates are reasonable, the fractionated REE distribution patterns of the alkali basalts (Fig. 2), compared with the relatively unfractionated patterns of the tholeiites, are broadly predictable (R. K. O'N. and K. Grönvold, unpublished) from solid-liquid partition coefficients⁸ for a source region composed of olivine+orthopyroxene+clino-

pyroxene+spinel, or the same with plagioclase instead of spinel. This is because the REE distribution patterns in the case of small amounts of melting will be strongly affected by crystal/liquid partition coefficients for cpx and opx, whereas in the case of more extensive melting olivine will become the major residual phase in equilibrium with the liquid. The absence of a significant Eu anomaly in these basalts favours generation at moderate pressures from a spinel bearing source, or from a plagioclase bearing source at comparatively high f_{O_2} (D. F. Weill and M. J. Drake, unpublished).

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Pleistocene River Gravels and the Stonehenge Problem

ON the basis of evidence of glaciation in southern England the presence of glacial ice and associated outwash in areas formerly regarded as ice free throughout the Pleistocene period has been suggested¹⁻³. In this account I examine the evidence in Wessex.

Kellaway¹ suggests that during both the "Anglian" and the Wolstonian glaciations, the rivers draining Salisbury Plain (Fig. 1) carried into the Hampshire Basin the outwash of great ice sheets which advanced towards Wessex from the Bristol Channel. He argues that in an "Anglian" glaciation the ice passed across Salisbury Plain and reached Romsey in the Hampshire Basin. Kellaway focuses attention on the distribution of the supposed erratics of the "Anglian" glaciation, and particularly on those erratics of medium-large size. He suggests that some of these have been incorporated into Stonehenge, and into other prehistoric monuments on the Wiltshire Chalk. He also suggests that the plateau gravels of the Hampshire Basin are the disturbed outwash of the "Anglian" ice, and that the valley gravels of the Wylfe, Avon and Test can be related to the dissection of the Chalk uplands in the two principal cold periods which followed the "Anglian" glaciation. These gravels must therefore include the outwash of the Wolstonian ice.

I now describe the composition of the Pleistocene gravels of the Wylfe, Nadder and Avon, with particular reference to the possible presence of erratic material of small size, and thus the possible relation of the river gravels to outwash of "Anglian" or Wolstonian age. Table 1 shows the composition of twenty-eight samples of gravel from twenty-four sites (mean sample size, 2,210 pebbles) (see Fig. 1).

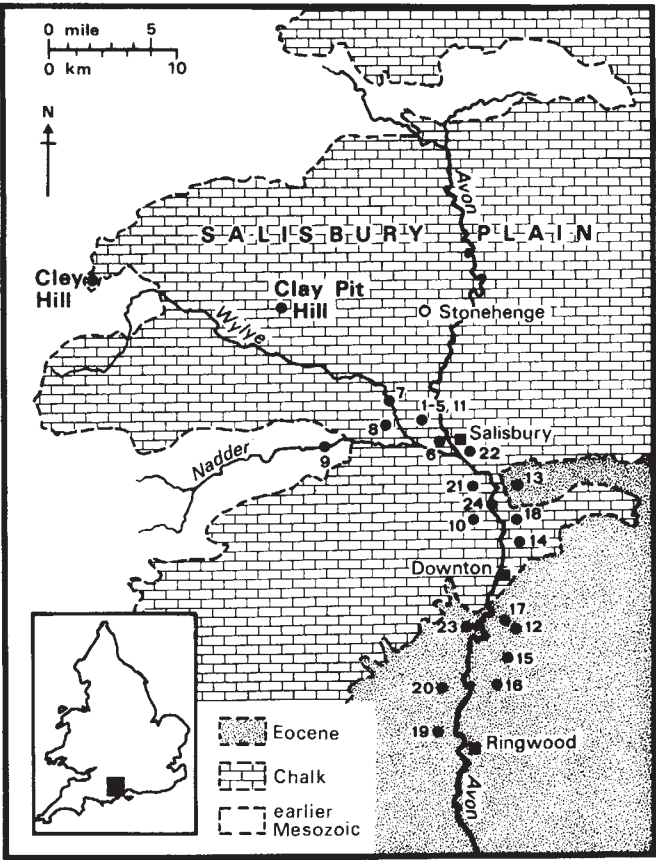


Fig. 1 Salisbury Plain and the Rivers Wylve, Nadder and Avon. Numbers refer to Table 1.

In age, the Pleistocene gravels seem to represent a great part, or perhaps the whole, of the Pleistocene period. The most elevated gravel, at Clearbury Ring (site 10) is at, or close to, the level of the end Tertiary land surface⁴. At a lower level the gravel of site 19 rests on what Sealy⁵ terms the Boyn Hill (Hoxnian) terrace of the Avon. Sites 6, 21, 22 and 30 might also be regarded, in terms of elevation and their contained Palaeolithic industries, as of Hoxnian age. Wolstonian outwash could therefore be present at or below this level. Above the Hoxnian level the chronological status of the gravels and terraces is less certain. The terraces of the Salisbury Avon have been discussed by various workers⁵⁻⁷, who seem to be agreed that a sequence broadly similar to that in the Thames valley is represented. Unfortunately, in the Thames valley, it is not agreed which terrace is contemporary with the "Anglian" glaciation. Both Wooldridge⁸ and Sparks⁹ have suggested, however, that the outwash of the "Anglian" glaciation at

its maximum extent was confluent with the Winter Hill terrace of the Thames. In any case, the "Anglian" glaciation is not likely to relate to a more recent stage than this. On the basis of the broad correlation indicated by Hare⁷, between the terraces of the Thames and those of the Salisbury Avon, the position of the Winter Hill and Boyn Hill stages is indicated in Fig. 2.

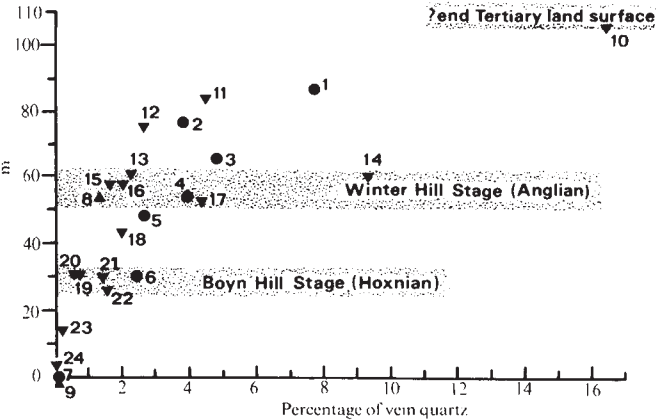


Fig. 2 Frequency of far travelled material (percentage of vein quartz) related to elevation of gravel above the present river (in m). Numbers refer to Table 1.

Several features relative to the composition of the Wessex gravels can be noted. First, the gravels seem to be derived exclusively from outcrops within the existing catchments; they are composed largely, or entirely, of Chalk flint and Upper Greensand chert or sandstone, material supplied from outcrops which are widespread in the basins of the three rivers. Other rocks are present in the gravels in very small amounts. Vein quartz, quartzites, grits and cherts occur. In terms of ultimate origin this material is far travelled; it is also present, however, in nearby Tertiary formations, and could be derived from them. Quartzose gravels, with pebbles of rock of western provenance, have long been known¹⁰ on the western margin of the Hampshire Basin, in the Reading Beds as far north as Downton, and in the Bagshot Beds near Dorchester. Outcrops of similar gravels have been described^{11,12} on Salisbury Plain, at Clay Pit Hill and Cley Hill. Thus, the immediate source of the far travelled material in the Pleistocene gravels can also be distinguished within the existing catchments.

Second, Fig. 2 shows that at no stage in the terrace succession in the Wessex area is there evidence of any significant change in the composition of the gravels. In particular, there is no indication of an influx of erratic material in outwash from either "Anglian" or Wolstonian ice. Far travelled material is most

Table 1 Composition (%) of Pleistocene River Gravels

Site No.	Wylve-Nadder							Nadder		Avon		12	13*	14	15	16	17	18	19	20	21	22	23	24
	1	2	3	4	5†	6*	7	8	9	10	11													
Height (m) above river	87	77	66	54	48	30	0	54	0	107	84	76	61	59	58	58	53	44	31	31	30	26	14	3
Flint and Greensand	91.7	96.2	94.8	95.7	96.1	96.1	99.9	98.6	99.9	83.6	95.5	97.2	97.8	90.6	98.2	97.9	95.6	98.0	99.3	99.4	98.6	98.5	99.8	100
Quartz	7.6	3.8	4.6	3.9	2.6	2.4	0.1	1.3	0.1	16.4	4.4	2.6	2.2	9.3	1.6	2.0	4.3	2.0	0.6	0.5	1.4	1.5	0.2	—
Other far travelled	0.7	—	0.6	0.4	1.3	1.5	—	0.1	—	—	0.1	0.2	—	0.1	0.2	0.1	0.1	—	0.1	0.1	—	—	—	—
Size range of sample	b	b	b	b	b	b	b	c	b	b	b	c	b	b	c	c	c	b	c	a	b	b	a	b

* Mean of two samples. † Mean of three samples.
Size range of samples: a, 6.3–25.4 mm; b, 4.0–12.7 mm; c, 3.3–6.3 mm.

frequent at the highest levels above the present rivers and progressively less frequent at lower levels. Assuming a broad correlation between the elevation and age of the gravels, this evidence seems consistent with derivation of the far travelled material from Tertiary formations. This Tertiary material, especially outliers on Salisbury Plain, would be progressively reduced in extent by erosion during the Pleistocene period, and would contribute correspondingly less material to successive Pleistocene gravels.

Finally, the Wessex gravels may be compared with those of the Thames. The influx of glacial outwash into the Thames basin on at least two occasions ("Anglian" and Wolstonian) is not in dispute. Walder¹³ shows that the mean content of glacially derived material in the Upper Winter Hill and Rassler terraces near Reading rises to as much as 30.7%. She relates this glacially derived material to the "Northern Drift", a till of the "Anglian" glaciation. Furthermore, much of the glacially derived material in the Thames gravels is of relatively large size. The figures obtained by Walder are based on a minimum sieve aperture of 3/8 inch (approximately 10 mm). Hey¹⁴, using a 16 mm minimum sieve aperture, also reports more than 30% "exotic" material in gravels of the Westland Green stage. In contrast, in the Wessex gravels the far travelled material is almost invariably small. In the coarse fraction (> 12.7 mm) of the gravels in Table 1, the mean frequency of far travelled material is 0.14%, representing a range from 0.0% (fifteen sites) to 1.16% (site 19).

The complete lack of glacially derived material in the Pleistocene river gravels of the Wylve, Nadder and Avon seems to show conclusively that the "Anglian" ice never invaded the basins of these rivers, and that the outwash of the Wolstonian ice was never tributary to them. "Anglian" ice cannot therefore be invoked to explain the presence of large and medium sized Welsh rocks, and other rocks of distant origin at Stonehenge, or in other prehistoric monuments on the Wiltshire Chalk. The problem of the introduction of these rocks to this area remains unsolved.

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"Burtle Clay" of Somerset

Kidson and Haynes¹ following Kidson^{2,3} claim that marine or estuarine deposits which they call "Burtle Clay" underlie the Burtle Beds of Somerset. The sands and gravels of the Burtle Beds have been described as marine deposits⁴ but a glacial origin has recently been ascribed to them^{5,6}. It is claimed¹ that the Burtle Clay is of considerable stratigraphical significance, that it proves the occurrence of a widespread marine transgression, destroys the case for the glacial origin

of the sands and gravels of the Burtle Beds, and throws doubt on the theory of glacial transport of the "bluestone" erratics incorporated in Neolithic and Bronze age monuments on Salisbury Plain. We find nothing to convince us of the marine origin of the Burtle Beds or to change our opinion about the older glaciations.

The Burtle Clay has not been described from any surface exposure, nor has six-inch mapping of the Burtle Hill area by the Geological Survey revealed any evidence of its presence. One "borehole" 3 foot (1 m) deep at Lilac Farm (Fig. 2 of ref. 1) is shown as having penetrated the Burtle Clay beneath a thin cover of sandy soil, but the material contains no foraminifera or ostracods and the description suggests to us a weathered tufaceous clay. We have not been able to find the precise location of borehole 8F and therefore cannot confirm the presence of tufa or "marl" in the area covered by the six-figure grid reference to Lilac Farm¹. The "borehole" seems to have been near the junction of peaty Holocene deposits and the sands of the Pleistocene Burtle Beds. Tufaceous clays commonly occur in this kind of situation in Somerset⁷⁻⁹. In any event there is no evidence to support a marine transgression.

South of the Poldens the only material which has been described as Burtle Clay is the red mud below the Burtle Beds in borehole 8K at Cutley Farm near Othery (Fig. 2 of ref. 1). Here it is shown as 1 foot (0.3 m) thick and presumably rests on red Keuper Marl which is the bedrock in this area. It seems to us most likely that this material is the weathered top of the Keuper Marl churned up by a drill bit or auger and contaminated by microfossils derived from the overlying Burtle Beds.

North of the Poldens the only boreholes showing any appreciable thickness of Burtle Clay are the shallow holes 8D, 8E and 8H (ref. 1). Two of these, 8D and 8H, passed through the Burtle Beds (sand and gravel) before encountering the problematical Burtle Clay which is shown as 7 to 12 foot (2.1 to 3.7 m) thick. No detailed comparison between the microfauna of the Burtle Beds and the Burtle Clay in the boreholes in the Burtle Hill area has been given by Kidson and Haynes¹, but most of the Quaternary foraminifera recorded¹ from the Burtle Clay are comparable with species which we have found in the Burtle Beds at Westonzoyland. In borehole 8E, where there is no overlying Quaternary sand and gravel, the Burtle Clay is apparently devoid of Quaternary foraminifera (Fig. 3 of ref. 1). The relatively low foraminifera counts in the Burtle Clay of 8D and 8H also suggest contamination. Yet neither the drilling equipment, the method of sampling, percentage recovery in the cores or samples, nor the possibility of contamination and remoulding of the material are mentioned. This information is important because remoulding and contamination of the Lias clay could give rise to samples such as those described as "doughy clay" (Fig. 1.6 of ref. 3). Alternatively this material could be till or head.

We have tried unsuccessfully to prove (by means of augering) the Burtle Clay shown in borehole 8E. Because of the inadequacy of the grid reference, we have been unable to locate the site of the three boreholes at Catcott Burtle. As two of the boreholes are shown in Fig. 1 of ref. 1 to be 500 yards (457 m) apart we presume there is a mistake in the grid reference.

The Burtle Clay of the Burtle Hill area is probably remoulded Lower Lias clay contaminated with microfossils or ground up shell debris (Fig. 1.6 of ref. 3), some having been carried by water draining from the overlying Burtle Beds in the boreholes. This, we think, may explain why the Burtle Clay seems to contain no Quaternary microfossils where there are no overlying Burtle Beds. The fact that no "Liasic" foraminifera occur at the top of the Burtle Clay in boreholes 8D and 8H suggests that they have been removed by weathering before the deposition of the Burtle Beds.

We have had difficulty in understanding the article by Kidson and Haynes¹. The key to Fig. 2 is incomplete, but we assume from the context that the broken horizontal shading

on the borehole sections indicates *in situ* Jurassic rocks. Then the presence of the Bucklandi Zone at the sub-drift surface in borehole 8G, and of "Middle and Upper Lias foraminifera" at the bottom of 8E, implies that a major fault, previously unknown, passes between two holes that are so close together that a single grid reference is given for both. As the Lower and Middle Lias in this part of Somerset is more than 1,400 foot (427 m) thick¹⁰ and the top of the Bucklandi Zone is about 300 foot (91 m) above the base of the Lias, it follows that the fault would have a throw of about 1,100 foot (335 m). This is roughly twice the displacement of any comparable post-Liasic fault known in the central Somerset basin and is not likely to have missed detection either by geological or geophysical surveys.

Of course the clays beneath the Burtle Beds at Burtle Hill might be weathered till or head, but it seems more likely to us that the samples examined by Kidson and Haynes¹ are remoulded Lower Lias clay. Whatever the explanation, it is clear that the presence in borehole 8E of clay with a Liassic microfauna but devoid of Quaternary forms cannot possibly be regarded as evidence for a Quaternary marine transgression. We also consider that the use of the term "Liassic" as applied by Kidson and Haynes¹ to the foraminifera listed under this title in Fig. 3 is misleading. Many of the genera have ranges extending from Permo-Triassic to Recent and one (*Ammodiscus*), Silurian to Recent¹¹. There may be Jurassic species of some of these genera in the Burtle Beds at Burtle Hill, and some of these could have become incorporated in the samples of Burtle Clay. But the genus *Planularia*, listed as "Liassic"¹, is usually considered to be Miocene-Recent¹¹ and we fail to see how it can be maintained that the only "time periods" represented by the microfossils of the Burtle Clay are exclusively "Lias" or Quaternary. Indeed, we deprecate the use of the term "Lias" in this context, as the total thickness of the Lower, Middle and Upper Lias before erosion may well have been of the order of 1,700 to 2,000 foot (518 to 610 m). Of this, only some 300 foot (91 m) of Lower Lias remains beneath the Burtle Beds at Burtle Hill. Any species of foraminifera of post-Sinemurian age, whether Jurassic, Cretaceous or Tertiary, found in the Burtle Clay could have been derived from the Burtle Beds but could not have been derived from the underlying Lower Lias.

We consider it regrettable that the term "Burtle Clay", based on such unsatisfactory evidence, should have been submitted for acceptance, particularly when attempts are being made through the Geological Society of London to improve standards of stratigraphical definition.

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Tidal Patterns and Energy Balance in the Gulf of California

Roden and Groves¹ wrote: "The oceanographic aspects of the Gulf of California, one of the least known marginal seas of the North Pacific, are both interesting and complex". Tidal processes certainly justify the statement. With a reported spring range of 10 m as well as currents exceeding 6 knots (ref. 2) the tides in the Gulf of California are among the most spectacular and dangerous in the world. Nevertheless, because of the rarity and inadequacy of observations, no consistent picture could be drawn until recently. Many quantitative studies of this area must thus be regarded as provisional; there is, for example, an eight-fold range in estimates of energy dissipation for the semidiurnal lunar tide, namely, Heiskanen^{3,4}: 2.5×10^{17} erg s⁻¹; Miller⁵: 4×10^{17} erg s⁻¹; Hendershott⁶: 1 to 2×10^{17} erg s⁻¹; and my proposed values: 4.35×10^{16} erg s⁻¹ for M₂ alone, 4.65×10^{16} erg s⁻¹ for all semidiurnal lunar constituents and 4.85×10^{16} erg s⁻¹ for all lunar constituents.

I have recently recorded tidal heights and temperature by means of self-contained pressure recorders laid on the bottom

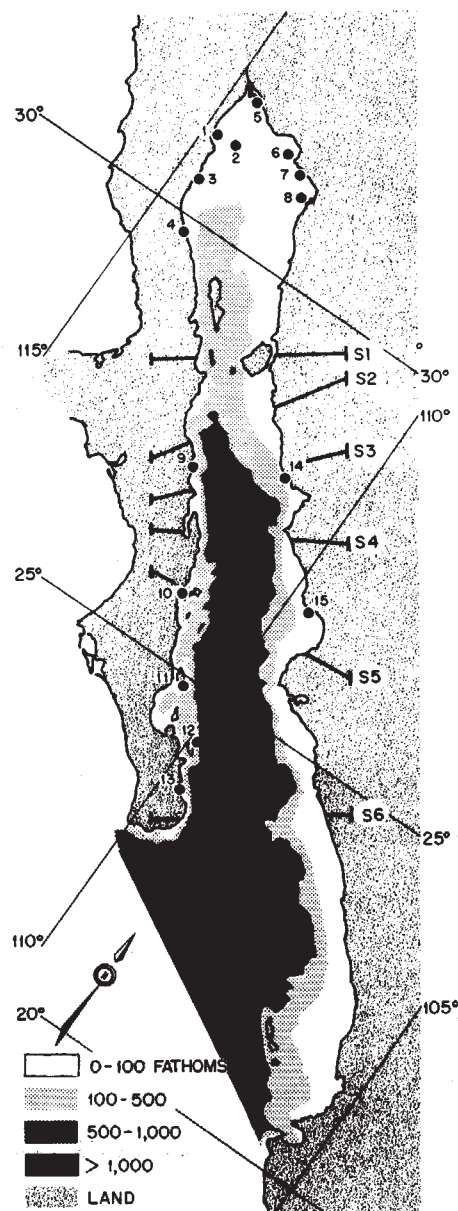


Fig. 1 Depth contours, identification and position of stations (1, 2 . . . 15) and identification of sections for which various estimates are tabulated (S₁, S₂ . . . S₆) for the Gulf of California.

at fifteen open coast or island locations at positions well removed from local influences (Fig. 1). The records cover 10 to 60 days, and with appropriate correction by convolution of the temperature trace with the instrumental response, are accurate to within about 1 cm.

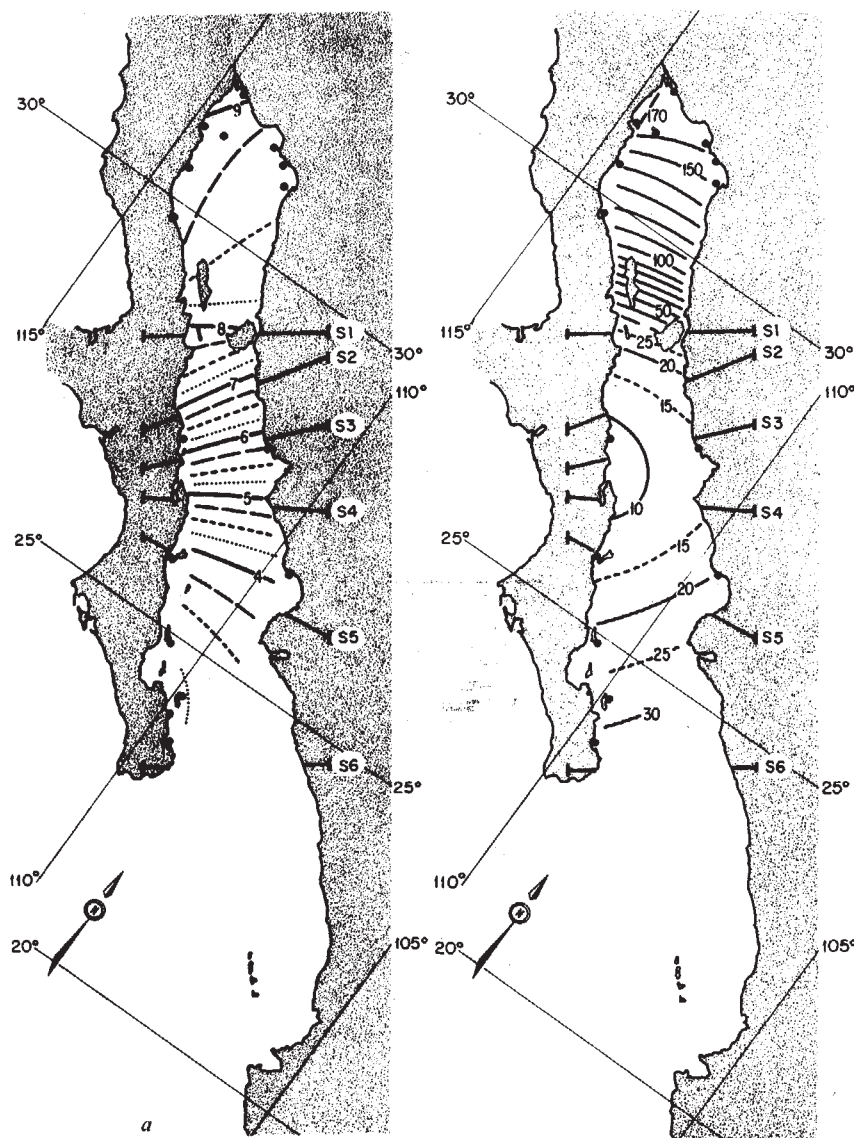
The resulting time series have been related to tidal driving forces through frequency dependent diurnal and semidiurnal transfer functions⁷. Amplitudes and phases for the chief diurnal and semidiurnal constituents K_1O_1 and M_2 , S_2 are listed in Table 1.

From the relatively dense data arrays resulting, cotidal and corange tide patterns can be readily sketched with remarkable certainty. When complemented with accurate—and available—coastal and bottom contour maps⁸ one may calculate accurately the tidal prism P_n or the volume of water above or below mean sea level to the head of a section n across the Gulf, the transport T_n , or rate of water inflow across section n , the potential energy distribution, E^p , the gravitational energy flux, E^g , or the energy contributed directly over the

Gulf by the gravitational forces of Sun and Moon as they enhance or hinder the flow of water from the adjacent Pacific Ocean, and the advective or oceanic energy flux E^o which represents the energy penetrating from the Pacific Ocean into the Gulf. With a smaller yet useful accuracy one may also calculate the average velocity v_n across a section n , the kinetic energy E^k associated with the moving water, and the ratio R of the amplitude of the tidal wave returning toward the ocean after reflexion in the Gulf to the amplitude of the entering wave. When gravitational driving is small compared to oceanic driving, R is called the reflexion coefficient. By insuring smooth variation of these quantities along the Gulf, yet preserving the fit of observations, I have obtained optimum tidal patterns.

Cotidal and corange maps for the principal semidiurnal constituent M_2 (Fig. 2) illustrate the behaviour of the semidiurnal tide. The diurnal tide is almost in phase (within 6°, or 24 min, Table 1) over the entire Gulf. It is, therefore, almost fully described by its corange lines (Fig. 3). The evidence is however, strongly in favour of the nearly axial orientation of

Fig. 2 Cotidal (*a*) and corange (*b*) maps for semidiurnal constituents M_2 . *a*, Labels on cotidal lines refer to lag in lunar h behind equilibrium tide at Greenwich; *b*, labels on corange lines are in cm.



the limited cotidal pattern shown in dotted lines at intervals of 4 min (Table 1, K_1 and O_1).

The difference between semidiurnal and diurnal tides is striking. The semidiurnal tide enters the Gulf with a moderate amplitude (30 cm for M_2), slows down inwards, decreasing to one-third its initial amplitude near the middle of the Gulf. It then accelerates while its amplitude rises to fifteen times its amplitude in the middle of the Gulf at the head (165 cm for M_2). There it seems to be absorbed by the coastline, suggesting large dissipation as also indicated by the shift into virtual existence of an amphidromic region near the middle of the Gulf as pointed out by Hendershott⁶. By contrast, the amplitude of the diurnal tide increases very slowly and monotonically to less than twice its amplitude at the mouth. Its nearly uniform phase suggests that it is like a standing oscillation, and that most energy entering the Gulf does leave again.

A summary of the calculations mentioned earlier is given in Tables 2 and 3. The complete estimates used, for convenience, are elements bounded by the coast and by cotidal lines for M_2 at intervals of 15 min (Fig. 2). They were obtained as follows:

$$P_n = \sum_n h_n \Delta A_n$$

where $\sum$ indicates a summation from 1 to n , h_n is the average

tidal elevation over the element n with area ΔA_n

$$T_n = dP_n/dt$$

$$v_n = T_n/S_n$$

where S_n refers to the area of the cross-section of the Gulf

$$E_n^g = \sum_n \langle T_n \nabla G_n \Delta l_n \rangle$$

where ∇G_n is the horizontal gradient of the gravitational potential, Δl_n is the mean tidal motion within an element n and the symbol $\langle \rangle$ indicates time averaging

$$E_n^0 = T_n h_n \cos \Phi_n$$

where Φ_n is the phase difference between transport and tidal elevation.

The time-averaged dissipation of energy D is the sum of the gravitational and oceanic energy fluxes, and the total energy stored in the Gulf is the sum of the potential and kinetic energy with

$$D_n = E_n^g + E_n^0$$

$$E^p = \frac{1}{2} \sum_n \rho g h_n^2 \Delta A_n$$

$$E^k = \frac{1}{2} \sum_n \rho \Delta A_n S_n V_n^2$$

where ρ is the specific gravity of seawater and g is the acceleration of gravity.

Table 1 Amplitudes and Phases (with Respect to Equilibrium Tide at Greenwich) of the Chief Diurnal Constituents K_1 , O_1 , and of the Chief Semidiurnal Constituents M_2 , S_2

Station identification	K_1		O_1		M_2		S_2	
	Amplitude (cm)	Phase (°)	Amplitude (cm)	Phase (°)	Amplitude (cm)	Phase (°)	Amplitude (cm)	Phase (°)
1	43.4	188	29.0	176	173.2	265	94.4	273
2	45.0	186	27.6	176	163.4	263	91.8	270
3	42.8	184	28.7	178	132.1	267	76.3	278
4	42.5	184	28.6	175	115.0	265	37.8	261
5	49.4	184	30.8	171	161.9	272	114.8	268
6	46.1	180	30.4	172	161.8	259	94.9	265
7	46.1	179	30.1	171	154.6	257	89.4	263
8	45.8	179	30.0	172	150.9	257	68.7	274
9	31.8	184	21.9	171	7.2	193	6.0	144
10	30.1	184	20.1	171	14.5	115	13.9	118
11	28.6	184	20.0	170	24.2	101	19.4	108
12	27.5	185	19.2	172	27.5	97	20.8	107
13	26.71	182	18.7	168	30.7	98	22.5	104
14	30.6	183	21.6	169	13.0	159	10.4	139
15	28.0	183	20.4	166	20.2	123	16.9	121

Table 2 Tidal Prism, Transport, Space-Averaged Peak Velocity, Energy Fluxes, Stored Energy and Reflexion Coefficients for M_2 Tide at Six Representative Sections

Section	Prism		Transport ($10^{12} \text{ cm}^3 \text{ s}^{-1}$) T_n	Velocity (cm s^{-1}) V_n	Oceanic ($10^{16} \text{ erg s}^{-1}$) E_n^o	Energy fluxes		Stored energy (10^{21} erg) $E_n^p + E_n^k$	Reflexion coefficient R_n
	Volume (10^{16} cm^3) P_n	Phase (°)				Gravitational ($10^{16} \text{ erg s}^{-1}$) E_n^g	Dissipation ($10^{16} \text{ erg s}^{-1}$) D_n		
S_1	5.53	258	7.77	38.8	3.66	-0.07	3.59	3.71	0.87*
S_2	5.74	257	8.06	23.0	4.08	-0.13	3.95	3.75	0.77
S_3	5.78	256	8.12	8.7	4.14	-0.17	3.97	3.75	0.72
S_4	5.77	255	8.11	5.6	4.26	-0.19	4.08	3.76	0.73
S_5	5.59	252	7.86	5.1	4.43	-0.22	4.21	3.78	0.75*
S_6					4.70	-0.35	4.35	3.80	

* Accuracy reduced because of small number of observations.

Table 3 Data* for K_1 Tides at Section S_6 (Oceanic Boundary of Gulf)

S_6	4.21	184	3.1	1.5	0.10	0.08	0.18	0.87	0.95

* Same column headings as for Table 2.

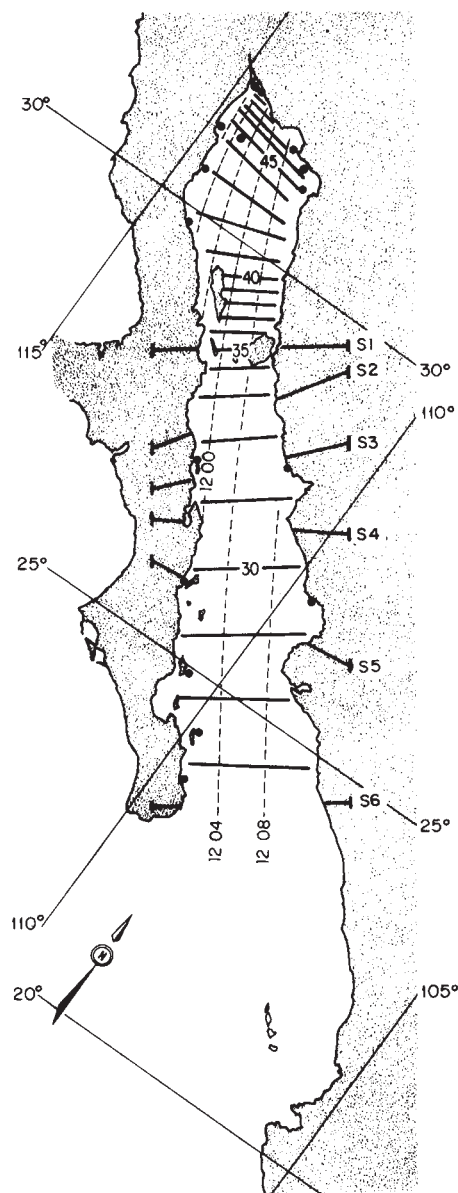


Fig. 3 Corange map for diurnal constituent K_1 . Labels on corange lines are in cm. Cotidal lines (---) are labelled in h and min behind equilibrium tide at Greenwich.

Owing to fanning out of the semidiurnal patterns south-east of section S5 and to the limited coverage of the observations, detailed calculations were not performed in this area for the semidiurnal tide. Nevertheless, semidiurnal dissipation can be extrapolated with confidence for the whole Gulf, assumed to be bounded by section S6.

At semidiurnal frequency the area of the Gulf to the north of section S4 may be regarded as a tuned quarter wave oscillator because the tide covers this space exactly one quarter of a tidal cycle. The southern half acts as an almost non-dissipative element connecting the northern half to the ocean (zero impedance).

A classical method of appraising the degree of damping—or dissipativeness—of a physical system is to estimate its resonant factor, Q . The latter is 2π times the number N of cycles required to dissipate an amount of energy equal to that stored in the system

$$Q = 2\pi N = 2\pi \text{ stored energy/dissipation per cycle}$$

For the northern Gulf, which resonates at semidiurnal

lunar frequency, $Q_{M_2} = 12.5$. This value rises slightly, to 13.2, for the entire Gulf. Hence, the tidal energy stored in the Gulf is dissipated in roughly two tidal cycles for M_2 .

The natural resonance frequency (F_R) of the whole Gulf is about 1.8 cycles day⁻¹ whether derived from wave propagation time with reflexion very near the most distant north-west shore, or from the phase relationships between waves entering and leaving. In the second case the effect of gravitational driving, which is negative for the semidiurnal tide and which amounts to about 5% of oceanic driving, has been neglected (Tables 2 and 3). The consistency of the results provided by the two methods confirms that most dissipation occurs in the shoals in the north-west of the Gulf as already indicated by Table 2.

The observed lag of S_2 with respect to M_2 (about 6) is consistent with the values of F_R and Q_{M_2} (ref. 9) if their oceanic phases are the same, a situation that seems plausible but cannot be fully established with existing data.

Calculations related to the much simpler diurnal tide are summarized in Table 3, for K_1 . Dissipation is smaller by comparison with stored energy than for M_2 ($Q_{K_1} = 30$); gravitational driving is positive and constitutes nearly as much as oceanic driving.

Table 4 Time Average Dissipation for Whole Gulf (10^{16} erg s⁻¹)

	Lunar	Solar
Diurnal	0.2	0.07
Semidiurnal	4.95	1.65
Total	5.15	1.72

Estimates of total dissipation, based on eleven constituents in each species, are given in Table 4, together with diurnal and semidiurnal, lunar and solar contributions. The total time-averaged lunar dissipation is 4.85×10^{16} erg s⁻¹, of which 4.35×10^{16} erg s⁻¹, or 90%, is associated with M_2 . The uncertainty in these figures, 10 to 15%, is chiefly a consequence of gaps in the observational coverage. One advantage of the present method is that it derives the required barotropic, or average, velocity field from precisely known time rates of water volume change. Thus baroclinic—or turbulent—motion is systematically eliminated, a requirement beyond the scope of available meters. The electromagnetic approach to estimation of barotropic motion, now almost operational and based on measurements on the seafloor of the electric field generated by water moving through the Earth's magnetic field, may soon confirm these estimates¹⁰.

Because tidal dissipation must have played an important role in the evolution of the Earth-Moon system¹¹, many attempts have been made to justify the agreement between estimates of tidal dissipation based directly on suspected dissipative mechanisms with those derived astronomically. With appreciable contribution from solid Earth tides dismissed on the grounds of high values for the resonant Q factor of the lowest Earth modes¹¹, attention is directed to the ocean. Important dissipation on the deep ocean floor, in spite of its dominant extent, is unlikely because bottom velocities are very low there, while friction in the case of a turbulent boundary layer follows a square law dependence on velocity. Thus, most tidal dissipation has been assumed to result from bottom friction in shallow and certain marginal seas with high tidal activity. Estimates of the dissipation in these areas have been attempted for more than half a century in spite of frustratingly inadequate data.

Present estimates for the Gulf of California are considerably lower than any previous ones. Such a situation arose earlier with respect to the Bering Sea^{4,12}. Should such a trend continue to be reflected in future investigations, dissipation mechanisms other than bottom friction of tidal currents may have to be ascribed an important role. If so, energy scattering into internal modes, followed by degradation into fluctuations of higher frequencies¹³, is the most probable explanation.

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Explosive Interaction of Liquefied Natural Gas and Organic Liquids

WHEN liquefied natural gas (LNG) is spilled onto water, a violent explosion may occur¹, caused by the sudden release of energy stored in a superheated, metastable LNG phase. It is therefore termed a superheat-limit explosion (SLE)^{2,3}. A SLE does not contain flames, but is very noisy. According to one theoretical estimate⁴, the explosion which can occur when LNG is spilled onto water is equivalent to 1/50 to 1/17 the explosion of a similar mass of TNT. Because the LNG would be spread over a much larger area, however, the force per unit area from such an explosion would apparently not be large.

SLE on water has been shown (Enger *et al.*, cited in ref. 3) to occur only when the LNG contains abnormally high amounts of heavier hydrocarbons (≥ 50 mol %); such a situation would not normally occur in practice and therefore SLE on water is not a point of great concern in the LNG industry. But in view of the very large number of LNG transport schemes now under consideration, which might involve the use of as many as 100 LNG tankers, it seems prudent to gain the best possible knowledge of how and why such explosions can occur.

We discuss here explosions which occur when LNG is poured onto various organic liquids. Occurrences of SLE on *n*-pentane have been mentioned before⁵, but for other organic liquids there is no published information. By sharp contrast with water, where a high concentration of heavy hydrocarbons in LNG is needed to cause SLE, explosions on some organic compounds occur even if LNG has its usual composition, less than 10% heavy components and more than 90% methane. The nature of the compounds which cause the LNG explosions gives useful information on the SLE mechanism.

The experimental arrangement was assembled in a well ventilated hood. An organic liquid (15 ml) to be tested was introduced in a 30 ml Pyrex beaker at room temperature. LNG (2.5 ml), taken from a Dewar with a pre-cooled plastic

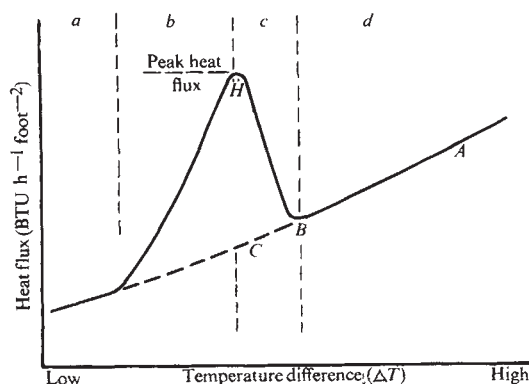


Fig. 1 Boiling heat transfer mechanism^{2,3}. Heat transfer modes: a, Convection and conduction; b, nucleate boiling; c, superheat region; d, film boiling. H represents the region of peak heat flux.

measuring spoon, was then poured into the liquid. Liquid spill-over, caused by the explosion, was collected in a water-filled tray placed below the beaker.

The gas from which LNG was prepared had the following composition (mol %): N₂ (1.5), CO₂ (0.2), CH₄ (90.1), C₂H₆ (5.4), C₃H₈ (2.1), *iso*-C₄H₁₀ (0.2), *n*-C₄H₁₀ (0.4), and *iso*-C₅H₁₂ (0.1). Phillips CP grade methane and ethane were used to prepare liquid methane and liquid ethane.

With LNG, the liquids which caused reproducible explosions were: *n*-pentane, *iso*-pentane, *n*-hexane, 2,2-dimethylbutane, 3-methylpentane, 2,3-dimethylbutane, 2-methylpentane, *n*-heptane and *n*-octane. With this group of liquids, the induction period was quite short; explosions occurred 2–3 s after pouring the LNG. Methanol, acetone and methylethyl ketone also caused reproducible explosions, but here the induction period was appreciable (about 5 s, sometimes longer). Explosions on ethanol and isopropyl alcohol were not reproducible, and occurred only in 5–10% of trials. No explosions were observed on *sec*-butyl-alcohol, *n*-nonane, *n*-decane or benzene.

By contrast with LNG, explosions with liquid methane were observed on *iso*-pentane, but on no other liquids.

Liquid ethane, poured into various liquids, simply vaporized instantaneously. With liquid ethane, SLE-type explosions were not observed on any of the liquids tested under the present experimental conditions.

Fig. 1 has been used before to discuss SLE on LNG-water systems^{2,3}. Here the rate of heat transfer from liquid to LNG is plotted against the temperature difference between cold LNG and the comparatively warm liquid below it. Initially ΔT is very high and the system is in the film boiling range. In this range, two liquid phases are separated by a thin vapour layer, and the heat transfer rate is quite low. As time elapses, ΔT decreases and the system approaches the superheat region (Fig. 1B–C). At this stage, the film collapses, two liquid phases come into contact and the rate of heat transfer suddenly increases. In certain conditions, this rapid heat transfer leads to the formation of a metastable, superheated LNG phase. Subsequently, stored energy is released explosively when the tensile strength of LNG is exceeded.

An important condition for the formation of the metastable state is that when the system is in the superheat region, the liquid below the LNG must remain free of any solid matter resulting from freezing. Otherwise, the solid provides nuclei for rapid boiling, and no superheating occurs. Freezing may result if the liquid has a high melting point⁶, or if heat transfer from the bulk phase to the LNG interface is too slow to compensate for heat loss there. Thus in a homologous series the higher members are more likely to freeze at the interface. On this basis, the occurrence of

SLE on C_5 , C_6 , C_7 and C_8 hydrocarbons but not on C_9 or C_{10} hydrocarbons is explained. Similarly, the occurrence of SLE on methanol but not on butanol is in accord with the expected trend. The somewhat longer induction period in methanol and acetone, by comparison with hydrocarbons, may arise because the solubility of LNG vapour in the former is low. This prolongs the period of film boiling because of poor heat transfer.

With liquid methane, the film boiling range is much longer than with LNG. Here it seems that most liquids freeze before, or while, superheating occurs. Only *iso*-pentane, which has the lowest melting point (-160°C) among the compounds investigated, remains liquid at the interface and leads to explosions.

With liquid ethane, instead of explosion, only rapid vaporization occurs. This is true even if the liquid ethane is cooled to -160°C before pouring so as to provide an initial ΔT which is comparable with that encountered in the experiments with LNG. It seems that, because of the high boiling point of ethane (-88°C), the system approaches the superheat region very rapidly ($<1\text{ s}$). When this happens, heat transfer from the bulk liquid phase may not be rapid enough to prevent freezing at the interface. It is hence not surprising that the metastable state is not formed in liquid ethane under the experimental conditions described here.

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Plumbosolvency of Waters

SOME cases of lead poisoning from domestic cold water supplies have been described^{1,2} and in a study of the lead content of water in Glasgow, where many older buildings have lead plumbing, a significant correlation was found between the length of lead pipe, the water lead content and blood lead of individuals resident in properties with lead plumbing³. In the following experiments a sample of old domestic lead piping was obtained to examine the effects of various factors on the relative plumbosolvencies of waters from various towns.

The lead piping used in the experiment was a typical piece of lead pipe (internal diameter 0.5 inch) which had previously been used for about 50 yr. Measurements of lead content were made after 1 h of contact of water with the pipe. Plumbosolvency is defined as the rate of solution of lead and is expressed as $\mu\text{g Pb l}^{-1}\text{ h}^{-1}$. The lead content of the water was determined on a Shandon-Southern A1660 polarograph. Calcium and magnesium levels were measured by atomic absorption spectrophotometry.

The pipe was longitudinally sectioned and its interior surface examined microscopically. It was found to be deeply fissured, with deposits of grey lead salts.

To investigate the effects of pH HCl and NaOH solutions were used, covering a range from 3–11. It was found that the plumbosolvency of the water increased considerably outside the pH range 6 to 8 (Fig. 1). Similar results were found for the water from the towns. A natural peaty water

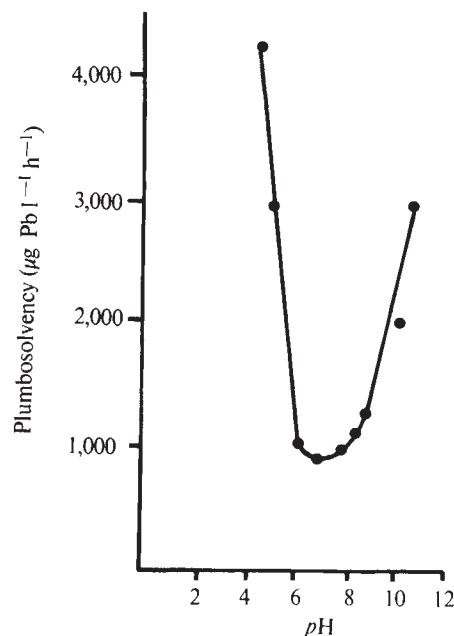


Fig. 1 Effect of pH on the plumbosolvency of distilled water. In this experiment the water was allowed to stand in the pipe section for 1 h with pH (adjusted with HCl or NaOH) being measured before and after this period. Temperature was maintained at 20°C .

was obtained from a moorland pool in the Kilsyth Hills. This water had a pH of 5 and, although as hard as the water from Edinburgh, it had a very much higher plumbosolvency.

There was a marked effect of temperature on the solution of lead from the pipes. As shown in Fig. 2 there is an exponential increase (plumbosolvency = $101 \exp(0.01T)$) in the plumbosolvency of the distilled water up to a maximum temperature examined (60°C).

Table 1 shows the measured parameters for the tap water from the various towns. The pHs lay between 6.2–6.6, similar to the pH of distilled water. The peaty burn water had a much lower pH of 5.1. Water hardness, assessed by the levels of calcium and magnesium in the water, was found to be in the order: distilled water < Glasgow < Edinburgh < Moorland < Paisley < London.

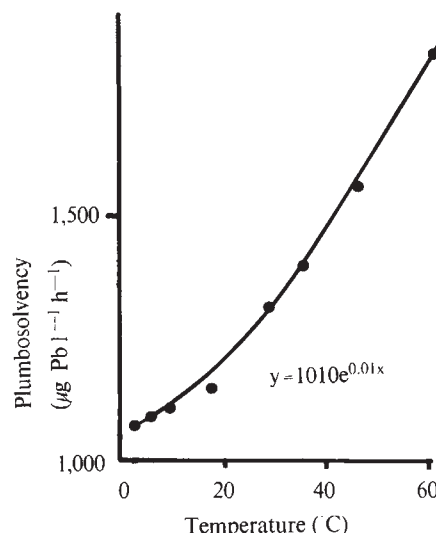


Fig. 2 Effect of temperature on plumbosolvency of distilled water. The temperature was maintained at the given value in a cold room if below room temperature or in a thermostatically controlled oven. pH remained constant at 6.2.

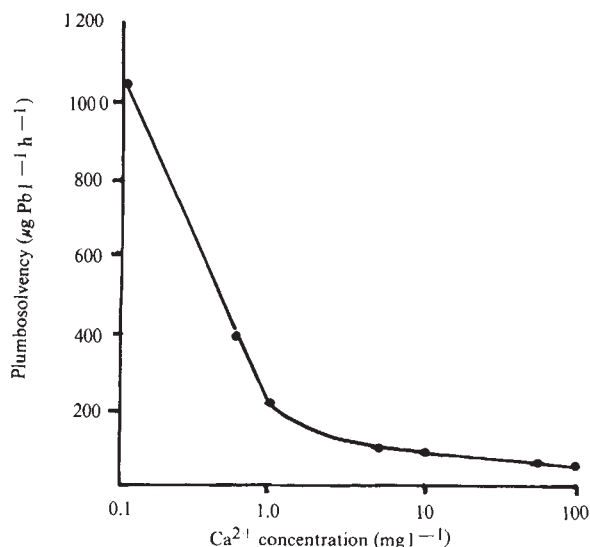


Fig. 3 Plumbosolvencies of synthetically hard water. Distilled water was hardened with calcium chloride to the concentrations shown. The pipes were kept at 20°C and in contact with water for 1 h.

In view of this, the plumbosolvency of synthetic hard water was examined. It was found that the relationship was a power curve where

$$\text{Plumbosolvency} = 306.3(\text{Ca}^{2+})^{-0.4}$$

It is in the region of normal water hardness, 0.1 mg l⁻¹ > Ca²⁺ > 10 mg l⁻¹, that there is a very rapid increase in plumbosolvency.

The effect of duration of flushing of water was determined in property supplied with water from lead piping. The length of lead supply pipe was 31 foot from the cast iron main. Lead levels in the water started at 272 µg l⁻¹ but rapidly fell to normal reaching an equilibrium level of 31 µg l⁻¹ after running the water for only 2 min at 12 l min⁻¹.

Table 1 Lead Corrosion Rates and Analytical Data for a Number of Waters

Water	Hardness		pH	Plumbo-solvency (µg l ⁻¹ h ⁻¹)
	Calcium (mg l ⁻¹)	Magnesium (mg l ⁻¹)		
Distilled	0	0	6.2	1,121
Glasgow	0.33	0.1	6.5	930
Paisley (Stanley filter)	1.15	0.24	6.6	462
Edinburgh	1.0	0.21	6.6	515
London	10.6	0.56	6.3	230
Moorland burn (Kilsyth Hills)	1.21	0.25	5.1	860

The use of lead pipes for cold drinking water supplies has been common since the days of ancient Rome and the health hazard has been known since that time. Beattie *et al.*³ showed that in Glasgow the levels of lead in water were greatly in excess of the WHO's recommended limit of 100 µg l⁻¹. Glasgow has a very soft water supply, derived chiefly from the natural reservoir of Loch Katrine. This reservoir, first suggested in the nineteenth century, raised some doubts as to the possible injurious effects of the increased plumbosolvency of the soft water. The opinions were sought in 1855 of Stephenson and Brunel. Their report stated that "Water of precisely similar quality and having the same power has been hitherto so used in contact with lead without any evil consequences"⁴. The techniques for lead determination at that time were insufficiently sensitive to detect the small, but possibly injurious, quantities of lead in the water.

Paisley has, since the 1930s, been supplied with water which is passed through limestone (calcium carbonate) Stanley filters. In 1960 part of the town was changed to a softer supply from Loch Katrine, thus providing the basis for a controlled study. Our results indicate that the plumbosolvency in those houses supplied from Loch Katrine is at least twice as great as those supplied from the Stanley filtered supply.

An additional safeguard in hard water areas is that a "scale" of calcium salts may form on the inside of the pipe, further reducing the plumbosolvency of the water⁵; in soft water areas corrosion increases the surface area available for lead solution. The other factors affecting plumbosolvency, temperature and pH, have little effect on cold water plumbosolvency, unless the water supply is acidified, for example from peaty moorland sources.

The hazard of lead exposure from domestic water supplies, though small, must be of some concern. The water as supplied by all local authorities in Britain is well within the WHO recommended limit of 100 µg l⁻¹ at source and indeed also at the tap in most cases. It is only when water is allowed to stand in lead pipes that a more concentrated lead solution forms. Thus it is important that in properties with lead piping, the "stale" or lead-contaminated water should be run away before domestic use; we have shown that 2 min flushing reduces the lead content to safer levels.

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Chemistry of the Conversion of Nitrate Nitrogen to Smoke Products

NITRATE nitrogen has been of interest to us because of its natural occurrence in burley tobaccos and of its use as a tobacco additive. Sodium nitrate added to tobacco is known to reduce particulate matter, nicotine, phenol, and benzo(a)-pyrene in cigarette smoke¹. It is thought that nitrates form oxygen and nitric oxide during smoking and thereby intercept radicals that would otherwise form polynuclear aromatic hydrocarbons. Nitroalkanes are known to increase in smoke as a result of the addition of alkali nitrate to tobacco and tobacco sheet², and catechol concentrations in smoke are known to be lowered if nitrate is added to tobacco³. A study which involved the fifth puff of a cigarette only, reports sodium nitrate to raise the levels of nitrogen oxides, acetaldehyde, nitriles and acrolein in the smoke⁴ (Table 1).

Nitrates labelled with ¹⁵N were used to follow the incorporation of nitrate nitrogen into smoke products. Cigarettes (1.14 g, 85 mm, non-filtered, 21 s porosity citrate paper) were hand-made from Kentucky Reference 1R1 tobacco which had been sprayed with aqueous solutions of the appropriate ¹⁵N-labelled nitrate [Ca(¹⁵NO₃)₂, 95 atom %; K and Na ¹⁵NO₃, 99 atom %].

Total nitrogen and nitrate nitrogen analyses showed the amounts of nitrate (as N) added to be 1.8, 1.5 and 1.8% for the calcium, potassium, and sodium salts respectively.

Table 1 Smoke Vapour Phase Constituents of Cigarette Smoke

Component	Control	μg (fifth puff) 8.3% NaNO_3
NO, NO_2	15.2	48.7
N_2O	1.0	11.0
HCN	13.2	23.3
CH_3CN	12.8	29.0

Data derived from Terrell and Schmeltz⁴.

The cigarettes were smoked to 30 mm butts under standard conditions; sidestream and mainstream smoke were collected separately. The components were separated by standard gas chromatographic techniques and analysed by low voltage mass spectrometry.

Table 2 Mainstream Nitrogen Oxides from ^{15}N -Nitrate

Nitrate	Unsmoked tobacco	Enrichment (%) N_2O	NO
Calcium	86	89	ND
Sodium	89.5	87	ND
Potassium	ND	ND	85

Table 2 gives the ^{15}N enrichments of the nitrogen oxides in mainstream smoke. The similar enrichments observed in the nitrogen oxides and the precursor nitrates indicate negligible dilution from other sources.

Table 3 Sidestream Ammonia from ^{15}N -Nitrate

Cigarette	Sidestream NH_3 (mg)	Calculated % of NH_3 from NO_3^-	^{15}N atom %
Kentucky ref. 1R1	5.3	—	—
+ $\text{K } ^{15}\text{NO}_3$	8.3	36	35
+ $\text{Ca } ^{15}(\text{NO}_3)_2$	9.0	40	33
+ $\text{Na } ^{15}\text{NO}_3$	10.2	48	42

Table 3 shows: (a) the increased ammonia deliveries from the nitrated cigarettes over the delivery of the reference cigarette, (b) the percentage increase in ammonia which resulted from the added nitrate, and (c) the corresponding incorporation of ^{15}N into the ammonia. These data demonstrate for the first time the conversion of nitrate nitrogen to ammonia during the smoking process. The conversion is highly efficient: for example 25% of the nitrogen added as calcium nitrate was converted to measurable sidestream ammonia; the absolute conversion, of course, is somewhat higher. The conversion to ammonia indicates that much of the smoke chemistry of nitrate may be the chemistry of intermediary ammonia.

Ammonia was supplied to a Kentucky Reference cigarette in the form of ^{15}N -glycine (2% N added by weight). The delivery of 13.8 mg (as against 5.3 mg for a control cigarette) of ammonia by this cigarette confirms glycine as an ammonia source. Table 4 shows that amides, heterocyclics and nitriles were highly enriched in ^{15}N . Thus, nitriles which were shown by earlier workers to increase if nitrate is added to filler can be derived from ammonia. Amides and N-heterocyclics are

Table 4 Smoke Products Derived from ^{15}N -Glycine

Smoke product	Delivery (μg cigarette)	^{15}N atom %
Ammonia	13,800	48
Acetamide	22	73
Pyrrole	263	54
Acrylonitrile	52	28
Hydrogen cyanide*	82	42

* Hydrogen cyanide data represent mainstream smoke. Other data are taken from the sidestream.

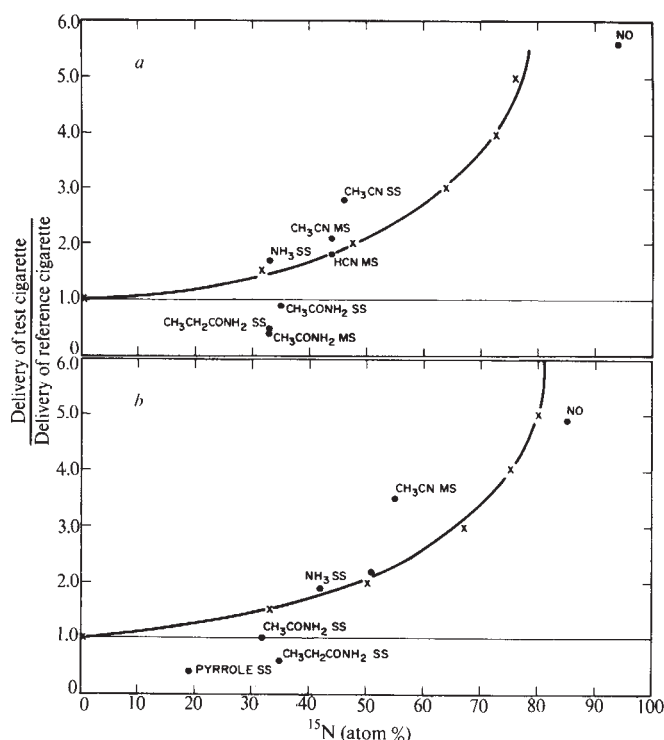


Fig. 1 Ratios of deliveries are plotted against ^{15}N uptake. Theoretical curve (—X—) would result if a 1:1 correspondence existed between ratios and uptake. SS and MS refer to sidestream and mainstream smoke respectively. a, Cigarette prepared with $\text{Ca}(^{15}\text{NO}_3)_2$; b, cigarette prepared with $\text{Na}^{15}\text{NO}_3$.

shown to be efficiently formed from ammonia and thus might be derived from nitrate as well.

Table 5 shows that the amides were highly enriched when cigarettes containing ^{15}N -nitrate were smoked. The enrichments were found to be equal to or less than the corresponding ammonia enrichments, suggesting that the amides result from: (1) reduction of $^{15}\text{NO}_3$ to $^{15}\text{NH}_3$; (2) reaction of the ammonia with a suitable intermediate to produce the amides. The N-heterocyclics were also significantly enriched but to a lesser degree than were the amides. This is compatible with the *a priori* assumption that there are non-ammonia routes to these substances.

Table 5 ^{15}N -Enrichments of Compounds Formed during the Smoking of Cigarettes Treated with ^{15}N -Nitrate

Compound	^{15}N atom %		
	Ca	K	Na
Formamide	34	—	—
Acetamide	34	25, 30*	33
Propionamide	34	24, 21*	38
Pyrrole	16	15	20
Methyl pyrrole	23	41	—
Dimethylpyrazine	17, 15*	17	24
Methyl pyridine	—	10	11
Pyridine	11, 13*	9	13
Hydrogen cyanide	39*	45*	58*
Acetonitrile	46, 45*	49, 42*	56, 56*
Acrylonitrile	35, 39*	39, 38*	56, 50*
Ammonia	33	35	42

* Asterisk numbers refer to mainstream smoke. All others refer to sidestream smoke.

Finally, the nitriles are shown to be dramatically affected by the nitrates; their enrichments being greater than those of ammonia, amides and N-heterocyclics, but less than those of the nitrogen oxides. Either or each of two processes super-

posed on reactions going on elsewhere in the cigarette may account for these observations. First, NH_3 may be generated at a favourable temperature such that nitriles (or suitable nitrile precursors) are formed before NH_3 mixing can occur. Second, nitric oxide which is highly enriched may react with alkyl radicals to give oximes which can dehydrate to give nitriles.

The ratios of yields of nitrogenous products from calcium and sodium nitrate cigarettes to yields obtained from the reference cigarette are plotted against ^{15}N uptake in Fig. 1. Data points would be on the theoretical curve if a 1:1 correspondence existed between product formation and ^{15}N uptake. These figures show the extent to which other factors cause differences in yields between the experimental and control cigarettes. The position of hydrogen cyanide in the graphs indicates subtle differences between the behaviour of nitrates. Sodium nitrate, for example, affected the puff count more than did the calcium nitrate, presumably because of a greater oxidizing action. Deviations from the theoretical line might also be due to depletions of the "co-precursors" of the nitrogenous products.

The ability of nitrates to contribute nitrogen oxides and oxygen to smoke is well known. Their ability to influence smoke product distribution *via* a highly efficient production of intermediary ammonia has now been demonstrated.

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BIOLOGICAL SCIENCES

Foetal Antigens and Cancer

EVIDENCE from several independent sources¹⁻⁸ suggests that the neo-antigens of tumours that excite transplantation immunity are reawakened or derepressed foetal antigens, that is, are differentiation antigens that make a transient appearance early in foetal life.

If foetal tissues do indeed contain antigens capable of exciting cell-mediated immunity (CMI) then the growth of foetal tissues in normal adult syngeneic mice should be inferior to their growth in immunosuppressed mice. Moreover the experiments of Coggin and his colleagues⁴⁻⁸ led us to expect that inoculation of foetal tissues into normal adult syngeneic mice would impede the growth of autochthonous or transplanted syngeneic tumours. For these reasons we decided to study the growth of foetal tissues in adult mice in various circumstances to learn whether an initial exposure to foetal tissue modified the behaviour of a second foetal implant or altered the growth of chemically induced or transplanted tumours.

Minced or mechanically homogenized tissues from 12-d-old CBA or C57 fetuses were implanted under the kidney capsule of adult mice. Recipients were killed at intervals and the sizes of the implants and the histological variety of tissues assessed.

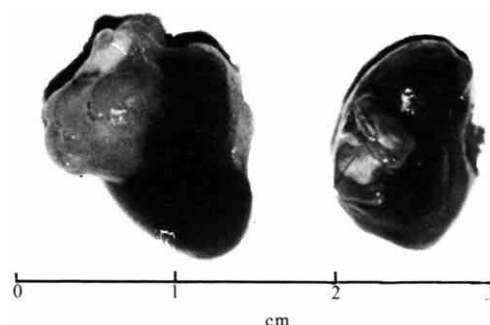


Fig. 1 Growth of homogenized 12-d CBA foetal tissues under the kidney capsule of normal adult CBA mice (right) or immunologically deprived CBA mice (left).

Recipients were either normal adult (8-12 weeks old), CBA males or thymectomized, lethally irradiated and bone marrow reconstituted ("deprived") mice of the same age, sex and strain. In some experiments recipients were "presensitized" to foetal tissues by the subcutaneous inoculation of foetal homogenates 2 weeks before the experimental implant.

Implants of both syngeneic and allogeneic foetal tissues grew poorly in normal adult mice. Implants of both types were about equally small and showed a restricted variety of tissues. In addition, a moderate lymphocytic infiltrate was observed in grafts of both kinds. The adult syngeneic tissue homogenates grew equally well in both environments, so it is unlikely that the two environments differed merely in their capacity to support growth as such. In deprived mice syngeneic and allogeneic tissues grew in great profusion to form embryomas (Fig. 1) in which most foetal organs or tissue types could be recognized (Table 1). Lymphocytic infiltration was absent.

Table 1 Histological Variety of Foetal Tissues growing under the Kidney Capsules of Normal Adult CBA Mice, Immunologically Deprived CBA Mice, and Normal Adult CBA Mice Pre-Treated with CBA Foetal Tissues

Normal mice Six mice	Deprived mice Six mice	Pretreated mice Six mice
Cartilage	Cartilage	Cartilage
Skeletal muscle*	Ossifying bone	Mature bone
Squamous epithelium*	Mature bone	Squamous epithelium
	Squamous epithelium	Hair follicles
	Hair follicles	Tooth
	Sebaceous gland	Kidney
	Eye	Ciliated epithelium
	Liver	Adipose tissue
	Ciliated epithelium	Pancreas
	Intestine	Skeletal muscle
	Brain	
	Marrow	

For details, see text.

* In one mouse only.

Superior growth of syngeneic foetal tissues in deprived mice was recorded in six out of eight sets of experiments but not in the other two; evidently some variable influencing the results has not yet been identified.

In normal mice which had received a previous implant of syngeneic foetal tissue, second foetal implants grew larger and in greater histological variety (Table 1).

An unexpected finding was that, far from suppressing tumour induction by 20-methylcholanthrene the previous treatment of adult mice with living syngeneic foetal tissues advanced the time of appearance of tumours (Fig. 2). Pre-treatment with foetal implants also enhanced the growth of a transplantable syngeneic tumour (Meth A) in normal adult Balb/c mice. This effect was more marked when irradiated

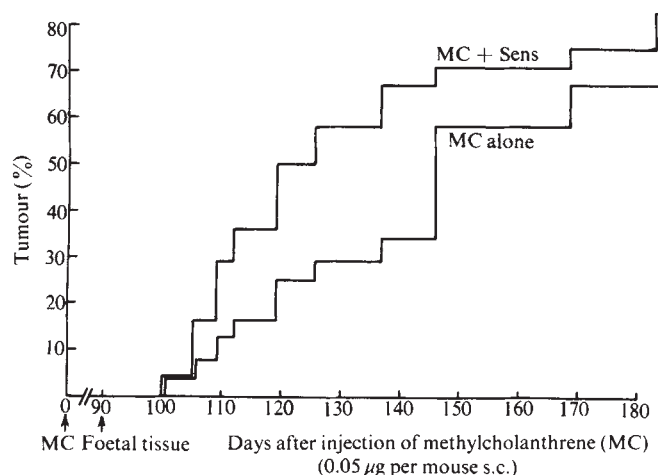


Fig. 2 Inception times of methylcholanthrene-induced tumours in normal adult CBA males, and in males pretreated with CBA foetal tissues.

allogeneic (C57) inocula were used (Fig. 3) and less striking with the use of non-irradiated syngeneic foetal tissue (Fig. 3).

These results suggest that syngeneic foetal antigens (differentiation antigens) may provoke a transplantation reaction in competent adult mice. Two observations accord with an immunological interpretation of these findings: the superior growth in immunologically deprived mice and after "presensitization"; correspondingly, the growth of transplanted tumours and the tumour induction rate after exposure to a chemical carcinogen were improved or "enhanced" in mice previously inoculated with foetal tissues. Although this was a result in the wrong direction from the standpoint of any possible therapeutic application, the modification of a second response by a first points to some immunological affinity between foetal and malignant tissues. In our models conditions have yet to be found in which tumour growth is inhibited. Irradiation of the foetal inocula was found to be essential by Coggin and

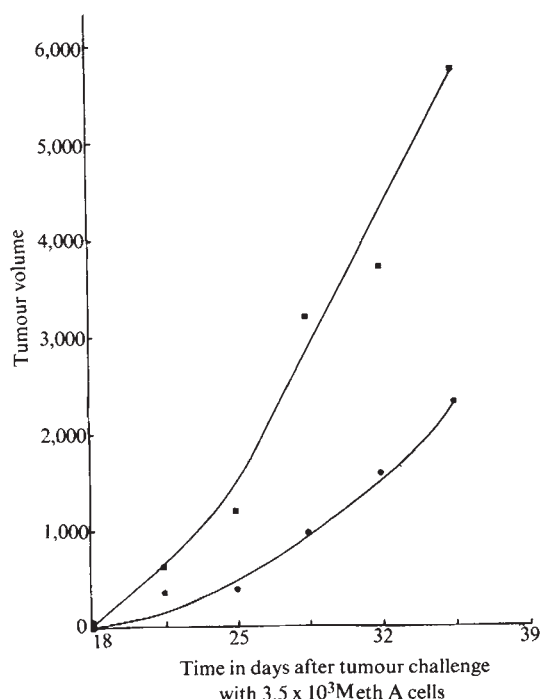


Fig. 3 Growth rate of 3.5×10^3 Meth A tumour cells in normal adult Balb/c mice (●—●) and in Balb/c mice pretreated with allogeneic foetal tissues (■—■).

his colleagues^{4,6,7} and Prehn^{10,11} has shown that in some circumstances a mild degree of immunity may enhance not only tumour but also foetal growth. These and other possibilities are now being investigated.

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Inhibition by Insulin of Cardiac Cathepsin D Activity

IN previous studies of foetal mouse hearts in organ culture, I have demonstrated that insulin improves net protein and amino acid balance¹ and prolongs the duration of survival and beating *in vitro*². The hormone exerts its action on amino acids such as phenylalanine whose metabolic pathways in the heart are confined almost exclusively to protein synthesis and degradation, as well as on those such as valine and alanine that may enter a variety of alternative pathways as well.

The effects of insulin on net protein balance could be a consequence simply of the well known ability of the hormone to improve protein synthetic capacity³. An alternative explanation is that insulin might inhibit the rate of protein degradation. Such an effect has been demonstrated in the liver⁴, and preliminary experiments by Rannels *et al.*⁵ have suggested that a similar action may occur in isolated hearts. No biochemical mechanism has been demonstrated, however, by which insulin can inhibit proteolysis. Perhaps there is a simple decrease in proteolytic enzyme activity, but there has been no evidence that the hormone has such an influence on any of the major proteases. Accordingly, I carried out the experiments described here to determine whether or not insulin has any effect on cathepsin D, a lysosomal enzyme that provides almost all the acid protease activity in the mouse heart⁶.

Intact spontaneously-beating hearts from 17 to 21-d foetal mice were maintained in organ culture. The hearts, each of which weighed only 1–3 mg, were pooled in matched groups of ten to provide sufficient material for the assays.

As before¹, cardiac weights and total protein content were significantly greater in hearts exposed to insulin; changes in the two parameters were proportional, so that the concentration of protein was similar in the control and treated groups (96 ± 2.4 (SEM) mg g⁻¹ in controls; 97 ± 3.6 mg g⁻¹ in those exposed to insulin). Activity of creatine phosphokinase, a myocardial enzyme that is not concerned with proteolysis and

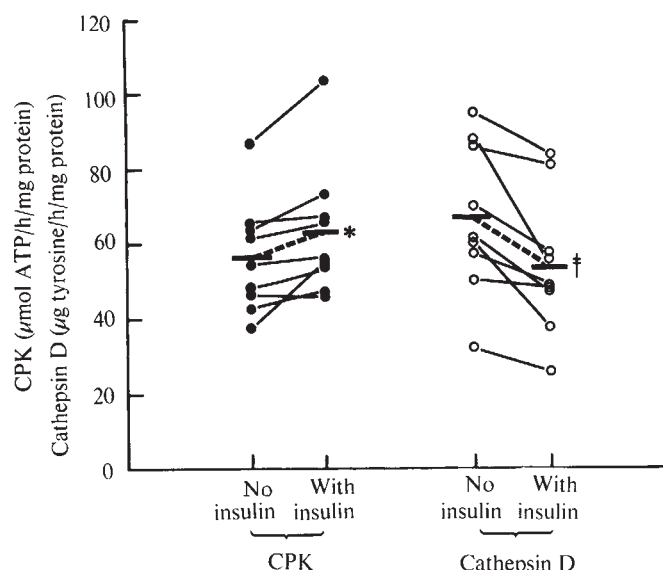


Fig. 1 Activities of creatine phosphokinase (CPK) and of cathepsin D in the presence or absence of insulin. One group of ninety hearts was maintained for 2 to 3 d in "medium 199" (ref. 7) with no insulin; matched litter mates were maintained in an identical medium supplemented with $50 \mu\text{g ml}^{-1}$ of purified bovine insulin. After 2 to 3 d the hearts were weighed, homogenized, and analysed for cathepsin D (ref. 8), creatine phosphokinase⁹, and protein¹⁰. Lines connect the values obtained in simultaneous assays of matched hearts. Horizontal bars denote mean values for each group. * $P < 0.05$ compared to matched group (Student's "t" test for paired observations); † $P < 0.01$.

that is not lysosomal in origin, was slightly greater in the insulin-treated group (Fig. 1).

In contrast, activity of cathepsin D was significantly higher in hearts that had been deprived of insulin (Fig. 1). In the absence of the hormone cathepsin D activity was $27 \pm 6.8\%$ greater than in its presence ($P < 0.01$). Separation of the homogenate in 0.25 M sucrose into a fraction that was bound to sedimentable particles and a fraction that was free in the supernatant¹¹ revealed that a greater change in enzyme activity occurred in the "free" fraction than in the "bound" compartment. The ratio of free to total activity, which is believed to reflect the fragility of lysosomal membranes, rose from $61 \pm 0.5\%$ to $64 \pm 0.5\%$ in the absence of the hormone.

These experiments demonstrate for the first time that insulin can affect cathepsin D activity in the heart. The effect of the hormone is to reduce the total synthesis or specific activity of the enzyme, and not solely to alter the physical characteristics of the lysosomes. Although the exact role of cathepsin D in mediating intracellular proteolysis remains to be established¹², the results suggest the possibility that the ability of insulin to improve net protein balance in hearts maintained for long periods *in vitro*, as well as its ability to prolong the duration of survival and function of isolated myocardium, may be related in part to a reduction in lysosomal proteolytic activity.

The present findings may be a curiosity of the specific conditions of the experimental system, and they may have little or no applicability to physiological or clinical problems. But the association of insulin deficiency with increased activity of lysosomal proteins and with increased proteolysis might well be a general phenomenon of wide importance. In this regard it is of special interest that starvation, which among its other effects causes hypoinsulinaemia, has recently been shown to be associated with markedly increased activity of cathepsin D in myocardial cells⁶. That observation, plus the present results, suggests that analyses of cathepsin D activity in other conditions associated with alterations in insulin levels *in vivo* would be important. Also, because preservation of isolated hearts and other organs constitutes an important problem in the field of clinical organ transplantation, the role of insulin

in inhibiting the activity of proteolytic enzymes deserves evaluation as a possible mechanism for delaying deterioration of organs maintained *in vitro* before transplantation.

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Milk Ejection during Alcohol Anaesthesia in the Rat

THE inhibition of oxytocin release by alcohol during labour and milk ejection has received wide acceptance and studies purporting to show this inhibitory function have been conducted on the rabbit¹, woman² and the rat³. In the last and most recent study, the litter weight gain was measured after a timed exposure to the alcohol-treated doe. Such criteria have, however, been outdated by recent developments. Milk ejection in the rat, and thus oxytocin release, is not the simple reflex process observed in other mammals⁴⁻⁹. In particular, milk ejection does not occur with the onset of nursing and, even when it does commence, it is an intermittent and an all-or-none event. Furthermore, no anaesthetic has so far been found which blocks this neuroendocrine reflex in the rat. In the light of these developments, therefore, I re-examined the alleged "ethanol block to oxytocin release" and found that the rat, completely anaesthetized with ethyl alcohol, displayed an almost normal pattern of milk ejection.

Adult rats were taken at day 10 of lactation and separated from their litters for 18 h (refs. 3-8). The does were then anaesthetized with 5 g kg^{-1} ethyl alcohol (12% solution) given by stomach tube or i.p. injection. When the righting reflex was abolished, usually within 15 min of the alcohol treatment, each doe was placed supine on the observation table and ten pups applied to the nipples. Milk ejection commenced, as judged from the behavioural activity of the young⁴⁻⁷, 50.9 ± 5.6 min after the alcohol administration or about 30 min

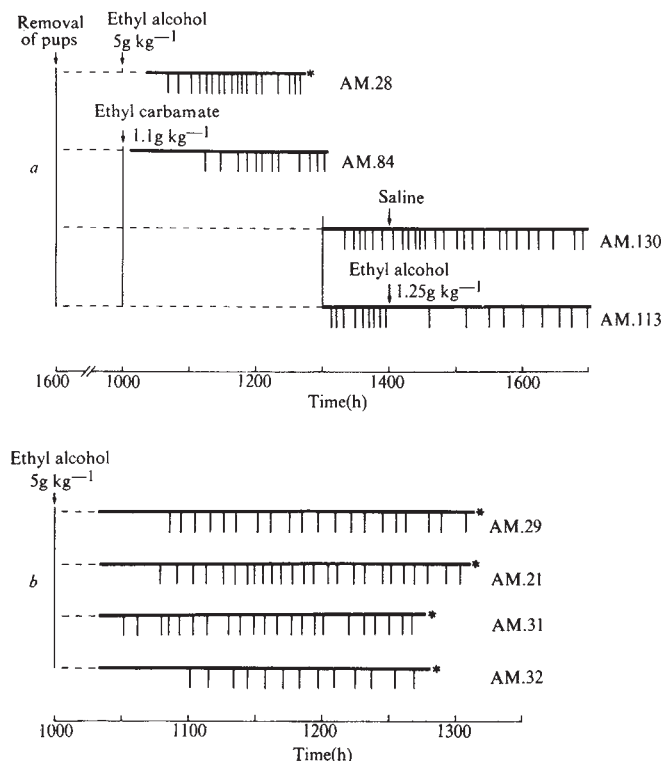


Fig. 1 *a*, Typical pattern of milk ejection is shown for each treatment; the code for the animal concerned is given on the right. The "thick" horizontal bar corresponds to the period of suckling and each vertical deflexion beneath this bar represents an individual milk ejection. *b*, Four other examples of milk ejection during anaesthesia with ethyl alcohol. In all cases of alcohol anaesthesia the recording was terminated by the doe attaining a righting reflex (*).

after the pups were applied to the nipples. Thereafter, a regular and intermittent pattern of milk ejection was observed for 2–3 h (Figs. 1 and 2). The mean milk ejection interval, calculated from the first milk ejection to the time at which the righting reflex was re-established, was 8.14 ± 0.73 min. The modal interval was shorter at 5–6 min as the distribution was made asymmetrical by the occurrence of a few rather long intervals of 20 min or more. On average 19.8 milk ejections were displayed by each preparation for a milk yield of 9.0 ± 1.2 g. This pattern of milk ejection is almost identical to that observed during anaesthesia with pentobarbitone sodium (20–40 mg kg⁻¹), ethyl carbamate (1.1 g kg⁻¹) and tribromoethyl alcohol (175–325 mg kg⁻¹) (ref. 6). Milk ejection in the

conscious doe differs in only two respects; the latency to the first milk ejection is shorter at 10.8 ± 3.1 min and the duration of nursing is limited by the doe to periods of 20–40 min. The mean milk ejection interval in the conscious doe, excluding the long periods when the doe was away from the nest, was 6.6 ± 0.6 min.

The behavioural response of the pups consists of a synchronized extensor or stretch reaction in which all the young suck and pull vigorously at the nipple for about 10 s. This is not the cue for oxytocin release, for it always follows, albeit within a second, the rise in intramammary pressure^{6–7}. Recordings of intramammary pressure were obtained during suckling of the doe under anaesthesia with ethyl alcohol at 5 g kg⁻¹ and this correlation between behaviour and intramammary pressure was confirmed (Fig. 2). Each milk ejection was the result of an extremely uniform release of oxytocin. In the example in Fig. 2 the release was 0.4–0.6 mU oxytocin, but in other animals a release of up to 1.5 mU oxytocin per milk ejection was observed. Fig. 2 also illustrates two other features of the milk ejection reflex of the rat which have not previously been described. First, the administration of exogenous oxytocin in physiological doses of 0.1–1.0 mU caused no observable change in the regular pattern of endogenous hormone release, by contrast with the many claims that exogenous oxytocin inhibits the reflex release of this hormone from the neurohypophysis^{10–15}. Second, there was a threshold level of intramammary pressure below which the behavioural response of the pups was not observed. Thus injections of less than 0.2 mU oxytocin failed to elicit a behavioural response though there was a distinct rise in intramammary pressure. The young presumably apply continuous negative pressure to the nipple and can taste milk after it arrives in the main teat duct at the time of milk ejection. It is only to be expected therefore that a small and rather transitory milk ejection will fail to elicit a behavioural reaction from the pups. The endogenous release of oxytocin is only two to four times the threshold for a behavioural response and it is feasible that on occasions the reflex release of oxytocin might fall below the threshold for an effective milk ejection. Such subthreshold releases of oxytocin, less than 0.2 mU, were not, however, observed during any of the pressure recordings.

Studies with less than the full anaesthetic dose of ethyl alcohol are complicated by several factors. With a conscious, though tranquillized, doe there is the problem of inhibition emanating from higher neural centres—the so called "cortical inhibition"¹⁶. It is also difficult to control the most critical factor in the milk ejection reflex, the suckling stimulus. These problems were largely overcome by the administration of ethyl alcohol to does already anaesthetized with ethyl carbamate (urethane). Anaesthesia with 1.1 g kg⁻¹ ethyl carbamate was most prolonged and 24 h after treatment the does were still

Fig. 2 Continuous polygraphic record of the intramammary pressure of a rat suckled when anaesthetized with ethyl alcohol at 5 g kg⁻¹. Reflex milk ejection (RME) is shown in series with milk ejection elicited by the intravenous administration of oxytocin, the dose of which is given in mU immediately prior to the milk ejection concerned. The time (min) is recorded from the application of the anaesthetic. Ten pups were applied to the nipples at approximately 90 min and the recording was subsequently terminated at 200 min by the doe attaining a righting reflex. The stretch response of the pups is signified by ● immediately beneath the pressure trace. The intramammary pressure associated with RME is equivalent to about 10–15 mm Hg.

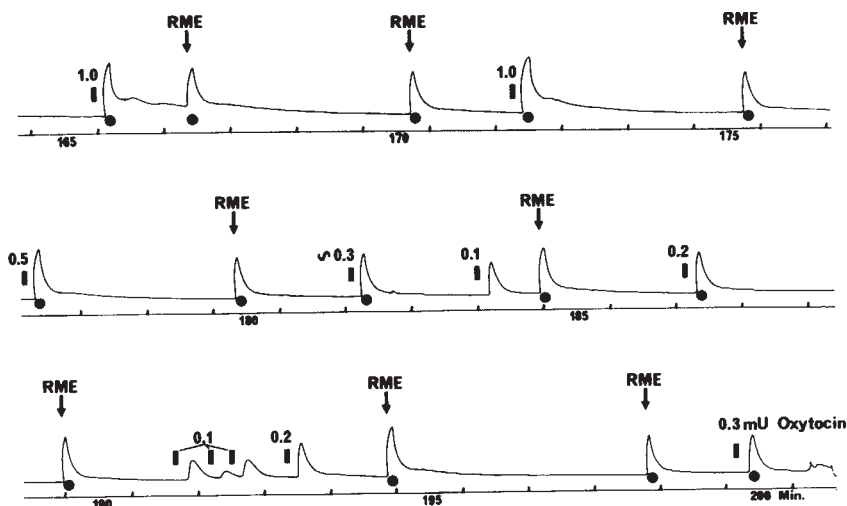


Table 1 Effect of Ethyl Alcohol on the Incidence of Milk Ejection*

No. of rats in sample	No. of milk ejections/rat in the 30 min before treatment	Treatment with ethyl alcohol g kg ⁻¹ , i.p.	No. of milk ejections/rat/30 min post-treatment					Total No. of milk ejections/rat	Weight gain/litter g
			0-30	30-60	60-90 min	90-120	120-150	150-180	
4	4.8	2.50	0	0	0	0	0	0.3	4.3
5	5.0	1.25	0	1.0	1.4	1.8	2.4	2.8	6.7
6	4.7	0.625	1.8	2.2	2.5	3.7	3.8	2.8	9.4
4	5.0	Saline	4.5	4.0	3.5	2.8	2.0	2.7	8.3
14	5.0	—	4.8	3.8	3.9	2.4			

* Applied to the nursing rat under anaesthesia with ethyl carbamate, 1.1 g kg⁻¹. 3 h later the pups were applied to the nipples and then, 1 h later, the alcohol was administered.

ataxic. The latency to the first milk ejection was 80.2 ± 6.7 min when the pups were applied within a few minutes of induction of anaesthesia, but this dropped to less than 20 min when the pups were applied 3 h later (Fig. 1). Doses of 0.625–2.50 g ethyl alcohol kg⁻¹ were then administered by i.p. injection to this latter preparation 1 h after the application of the young, that is after the occurrence of four to ten milk ejections. The effect of the 2.5 mg treatment was to inhibit milk ejection in the subsequent 3 h of suckling (Table 1); with the 1.25 and 0.625 mg treatments the block was less complete and the first milk ejections occurred after 66 and 8 min, respectively. The frequency of milk ejection then increased with time and after 2–3 h the frequency of milk ejection was greater than that observed in the control preparations.

My study does not completely contradict the former work on the interaction between alcohol and oxytocin release. If I had applied the criteria of Fuchs³, that is, the weight gain of the litter after 30 or 60 min exposure to the alcohol-treated doe, an increase of weight would not have been observed in most preparations. An almost normal pattern of milk ejection, however, occurred throughout the remaining 2–3 h of anaesthesia. The increased latency to the first milk ejection, compared with conscious preparations, is not specific to alcohol, for it is seen with all the anaesthetic situations so far studied⁶. It is possibly related to the metabolism of the drug concerned for, first, it is dose dependent, and, second, it is reduced to the value observed in the conscious doe when the young are applied sometime after the onset of anaesthesia. Conversely, milk ejection is not confined to a short period coincident with anaesthetic recovery. Considerable evidence is available to suggest that full anaesthesia is a prerequisite for milk ejection under the experimental circumstances which have been studied. Milk ejection was never observed during periods in which the doe displayed positive signs of consciousness, for example, head, limb and whisker movements. The cortical electroencephalogram (EEG) was recorded from both alcohol and urethane treated does: milk ejection occurred during slow-wave conditions (synchronized-EEG¹⁷) but never during arousal (desynchronized EEG). One possible explanation is that full anaesthesia, as expressed in the form of a sleep-like EEG, eliminates the cortical inhibition of the milk-ejection reflex which would otherwise operate. The possibility of there being a stress effect, involving the pituitary-adrenal or sympathetic-adrenal systems, is slight, for most animals are severely stressed throughout the period of anaesthesia, and this is particularly true after the administration of urethane¹⁸.

I conclude therefore that the "ethanol block to oxytocin release" is seen as a relatively ineffective and non-specific phenomenon.

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Lamprey Growth and Biological Conditions in the Bristol Channel Region

IN view of public disquiet over the possible deterioration of some of our rivers and coastal waters, including the Severn estuary, it is encouraging to find some evidence, although indirect, that as far as the populations of migratory lampreys are concerned, conditions in the area of the Bristol Channel appear to be at least as favourable for their survival and growth as was the case ten years ago.

In those major river systems in which they occur, the anadromous lampreys are generally much more numerous than the migratory salmonids and, for this reason alone, are a more useful biological indicator of changing conditions in the estuarine and coastal environment. As predaceous animals feeding on a wide variety of teleost fishes it would be anticipated that the rate of growth of the adult lamprey would, to some extent, reflect the abundance and accessibility of these host species within their feeding area. This is borne out by experience in the Great Lakes of North America where the body size of spawning migrants of the landlocked sea lamprey, *Petromyzon marinus*, has proved to be a sensitive index of the status of the host fish populations¹. Following the introduction of these parasitic lampreys into the Upper Great Lakes, their body size has tended to decrease over the period of time for which adequate records are available and this has been most marked in Lake Huron, where the average weight and length decreased by 35% and 13% respectively between 1947 and 1960. That this decrease in body size was at least partly attributable to a depletion of host fishes is suggested by the marked decline in the stock of lake trout (*Salvelinus*

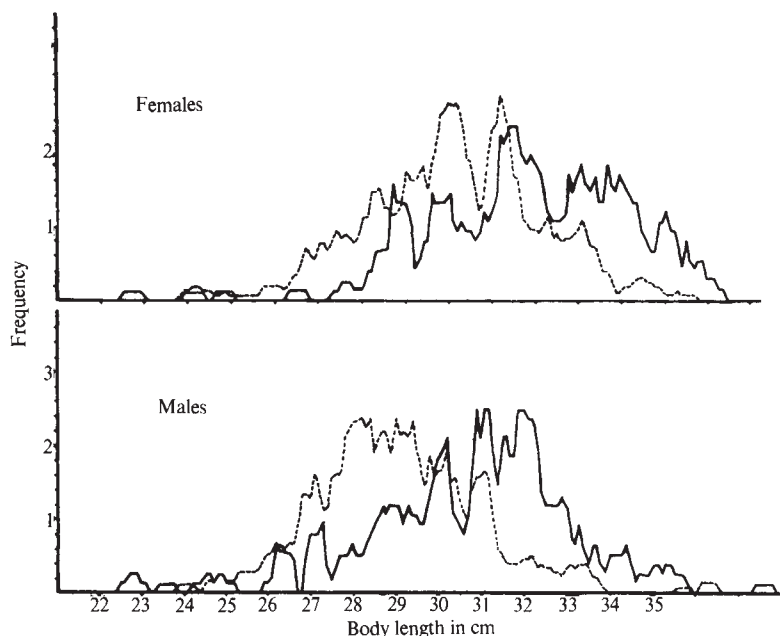


Fig. 1 Length-frequency distributions for male and female river lampreys (*Lampetra fluviatilis*) taken from the River Sever in October 1962 and 1971. (Smoothed by moving average of 5 mm.) ---, October 1962; —, October 1971.

namaycush), burbot (*Lota lota*) and whitefish (*Coregonus clupeaformis*) that was already apparent before the maximum number of lampreys was reached in 1949.

Insofar as they reflect a dynamic interrelationship between changing parasite and host populations within a closed lake system, these changes may be regarded as exceptional. Nevertheless, they indicate that the final body size attained by the sexually mature lamprey is mainly a reflexion of feeding opportunities and that a deterioration in feeding conditions and growth rates is apparently not accompanied by an extension of the parasitic phase of the life cycle.

The upstream migration of river lampreys (*Lampetra fluviatilis*) in the Severn generally begins in late summer or early autumn, reaching a peak in October or November. In the autumn of 1962 and 1971, records have been made of the lengths and weights of more than 1,300 river lampreys (Table 1), most of which were caught at Tewkesbury. The length (Fig. 1) and weight frequency distributions show the wide dispersion and polymodality characteristic of this species² with lengths varying from 22.7 to 37.6 cm and weights from 19.3 to 105.0 g. For both sexes, the body size of the 1971 migrants was conspicuously larger than in 1962. Thus, the mean length for males caught in October 1971 was 30.6 cm compared with only 29.0 cm in 1962, whereas for females the corresponding values were 31.8 cm and 30.1 cm, representing percentage increases of 5.5% and 5.6% respectively. Differences in body weight are even more striking; for 1971 the mean weight for October females is 69.1 g compared with 58.0 g in 1962; for the males the corresponding figures are 59.1 g and 51.9 g, giving percentage increases of 19.1 for females and 13.9 for

the males. Similar differences were also observed in the November material. For each of the corresponding groups in Table 1 the differences in the means are highly significant, with probabilities in no case greater than 0.001.

It is difficult to assess how far changes in body size might be a result of fluctuations in the relative abundance of the lamprey population and consequent changes in the pressure on host fish populations. Nevertheless, such evidence as we have suggests that this could not account for the difference in body size of the 1962 and 1971 populations; indeed, according to reliable local observers, the 1971 run was one of the largest within recent years; a view substantiated by the number of lampreys caught.

The precise limits of the feeding area of the river lamprey are unfortunately not known. After studying their osmoregulatory ability in seawater, Bahr³ has concluded that in the North Sea coastal areas this species is predominantly an estuarine feeder, although more recent work on the downstream migrant stages has indicated that in many cases they can be acclimated to full strength seawater^{4,5}. In the Mediterranean, Zanandrea⁶ obtained river lampreys within a few km of the coast in the area of the Gulf of Gaeta and all the indications from the North Sea and the Baltic suggest that this species probably feeds in the coastal regions². Thus, it seems likely that the populations which migrate into the Severn estuary are probably drawn from the area of the Bristol Channel and its immediate offshore shallow waters, and that in these regions the supply of suitable host fishes is at least as adequate as in 1962. In this connexion a recent report⁷ has indicated a significant revival in the stocks of certain teleosts in the

Table 1 Mean Lengths and Weights of River Lampreys in 1962 and 1971

	Number	Males		Number	Females	
		Length, cm. Mean $\pm$ s.e.	Weight, g Mean $\pm$ s.e.		Length, cm. Mean $\pm$ s.e.	Weight, g Mean $\pm$ s.e.
October 1962	340	29.0 ± 0.1	51.9 ± 0.6	348	30.1 ± 0.1	58.0 ± 0.6
November 1962	43	29.0 ± 0.3	48.5 ± 1.4	70	29.8 ± 0.2	54.6 ± 1.3
October 1971	153	30.6 ± 0.2	59.1 ± 1.2	154	31.8 ± 0.2	69.1 ± 1.2
November 1971	150	31.5 ± 0.2	55.0 ± 1.0	150	32.5 ± 0.2	62.4 ± 1.1

English Channel, in particular, the cod (*Gadus morhua*), ling (*Molva molva*) and to a lesser extent the herring. Should similar changes have occurred within the areas adjacent to the Bristol Channel they may well have produced conditions more favourable to the growth of the river lamprey. This seems to conflict with conclusions reached in a recent survey of coastal sport fishing in Britain⁸, which suggested that the Bristol Channel area and its approaches had suffered a greater decline in the variety of fish than most other coastal waters. The explanation may be that the adverse effects of pollution are more apparent in the immediate coastal zone (where angling is generally carried out), rather than further offshore where the lamprey is believed to feed.

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Comment on "Visual System's View of Acoustic Space"

MORRELL¹ has presented "evidence of a rigorous spatial specificity in the acoustical projection to visual neurones". He observed that "along the horizontal meridian, the acoustical receptive fields were arranged in as orderly and systematic a way as the receptive fields for vision". The acoustical receptive fields were mapped onto visual receptive fields "having the same horizontal boundaries as the corresponding visual receptive field(s)" (Fig. 4, ref. 1). The superposition of the visual and auditory receptive fields is difficult to understand if one takes into account that the eyes move within their orbits. What happens to this superposition if, for instance, the eyes are turned 10° to the left side whereas the head remains in its original position? Three different results are conceivable: (1) The visual receptive field is shifted 10° to the left and thus keeps its position with reference to retinal coordinates. The auditory receptive field remains at its original position. In this situation there would be a dissociation between the position of the visual and the auditory receptive fields. (2) Both the visual and the auditory receptive fields are shifted 10° to the left, that is, the visual system would "control" the position of the receptive fields of the auditory system. (3) Both the visual and the auditory receptive fields remain at their original position, that is, the position of the visual receptive fields is remapped in order to coincide with the position of the auditory receptive fields, and thus with head coordinates. The outcome of an experiment investigating these possibilities cannot be anticipated, and the results might differ between an anaesthetized and an unanaesthetized preparation. It seems, therefore, that the data do not yet establish clearly that there is a

"rigorous spatial specificity in the acoustical projection to visual neurones".

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¹ Morrell, F., *Nature*, **238**, 44 (1972).

Reply

COMMENTING on my communication "Visual System's View of Acoustic Space"¹, Pöppel has made two observations. The first argues that I have not demonstrated a "rigorous spatial specificity in the acoustical projection to visual neurones" as that statement does not take into account what may happen when the visual axis is displaced from the auditory. The second comment suggests three different findings which such displacement might yield. Pöppel admits that there is little basis for prediction among these possibilities and that the result might differ depending on whether or not the animal was anaesthetized.

The questions are good. The answers depend on further experimentation. My conclusion was based on findings in a particular and well defined experimental situation; one in which the acoustical and visual axes were aligned. Given those restrictions, I feel the conclusions to be warranted. I do not extend them to all and every circumstance. Given the experimental situation as defined, the data seem to have an extraordinary regularity—one which is in every way as regular as the receptive field properties for visual stimuli.

Experiments to answer the questions posed by Pöppel are not yet complete. Preliminary findings indicate, however, that shifting the acoustical axis, for instance, by closing one ear, while maintaining fixed retinal coordinates results in a shift of the acoustical receptive field but not the visual. Thus, the third possibility mentioned by Pöppel does not obtain; the acoustical system does not "control" or remap the visual receptive field. Nor does the reverse occur. Fixation of retinal coordinates does not impose similar fixation of acoustical fields. Dissociation, that is, the first of Pöppel's alternatives, seems to hold in the paralysed, unanaesthetized cat.

The implication of this finding with regard to the behaving, unrestrained animal remains to be determined.

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¹ Morrell, F., *Nature*, **238**, 44 (1972).

Attached *Sargassum* Found in Britain

ON February 17, 1973, some thirty scattered plants of the brown algal genus *Sargassum* were found attached on a sheltered shore in Hampshire. This is the only record for attached plants of a species of *Sargassum* in Britain. The specimens were well grown, attaining a length of up to 1 m (Fig. 1), and showed the characteristic morphology of this genus with monopodial branching, air-bladders and leafy laterals. We are confident that these plants constitute a recent introduction as the area has been under surveillance since 1967, being one of the localities for a possibly-introduced *Grateloupia* sp. (red alga)¹.

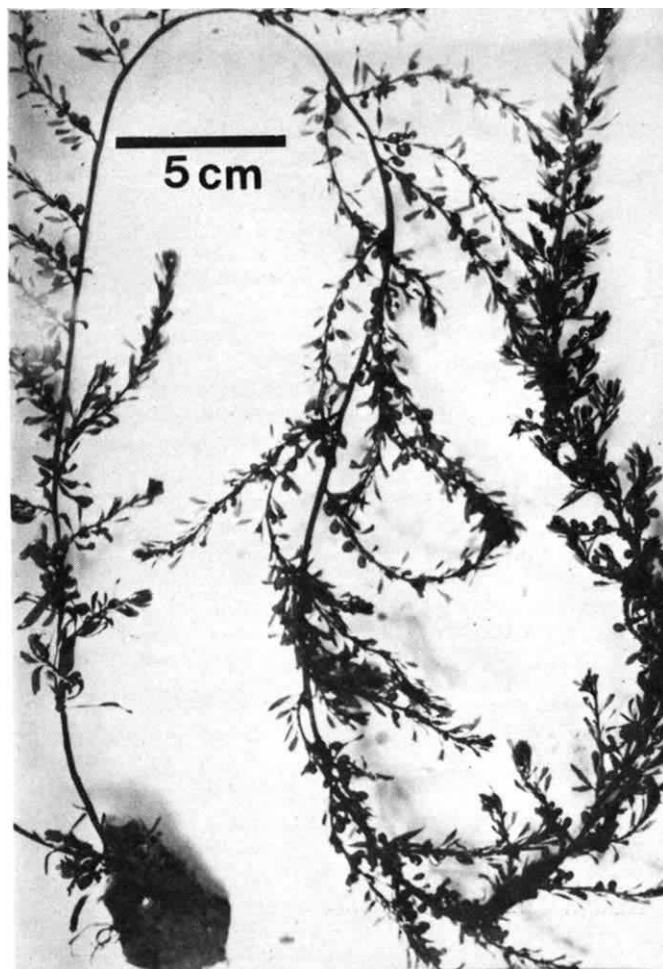


Fig. 1 Photograph of an entire attached plant of *Sargassum muticum*.

Sargassum is a taxonomically complex genus accredited with up to 250 species. From the available literature and herbarium specimens our plants were identified as *S. muticum* (Yendo) Fensholt. The occurrence of this species raises wider issues than merely the addition of another species to the British marine flora. Scagel² reported the accidental introduction of *S. muticum* from Japan into the northeastern Pacific with spat of the Japanese oyster (*Crassostrea gigas*). This alien weed is now distributed from British Columbia to California^{3,4} and may be displacing the ecologically important local eel-grass (*Zostera marina*)⁵. Our *Sargassum* population is similarly associated with a *Zostera* bed. The outbreak of "wasting disease" of *Zostera* in the North Atlantic during the 1930s resulted in many important members of the *Zostera* community also being affected⁶. The implications of the introduction of a potentially serious competitor to the *Zostera* are self-evident.

Druehl⁵ recently drew attention to the increasing practice of long-range transplantation of marine organisms, particularly those of commercial importance. He referred to the general lack of international control over this, despite inability to forecast reliably the effects of alien species on indigenous communities. Druehl also predicted the establishment of *S. muticum* in the eastern Atlantic as a result of direct importation by air of *C. gigas* from British Columbia into French waters. The accuracy of his prediction has been substantiated by the discovery reported here, made during the month following publication of his letter.

Spat of the Japanese oyster have been imported in bulk into French waters since 1966 from both Japan and British Columbia⁷. Importation of all molluscan shellfish into Britain is limited under the Molluscan Shellfish (Control of Deposit)

Order of 1968, No. 1164. As far as is known, no *C. gigas* has been imported into British waters except under quarantine hatchery conditions; these were applied to the *C. gigas* spat reared in Chichester Harbour⁸.

It is not possible therefore to conclude at present that the deliberate commercial spread of the Japanese oyster has been responsible for the accidental dispersal of *S. muticum* into the Channel, although this type of situation was envisaged by Elton⁹, "But the greatest agency of all that spreads marine animals to new quarters of the world must be the business of oyster culture. . . ." Certainly a number of undesirable predators, for example, the American oyster-drill (*Urosalpinx cinerea*)¹⁰, and competitors such as the slipper-limpet (*Crepidula fornicata*)¹¹, are known to have been introduced along with oysters.

Alternatively, other agencies, such as shipping, may have been responsible for the sudden appearance of *S. muticum*. Previous introductions into the Solent area which have become established include the Australasian barnacle *Elminius modestus*¹² and the Japanese sea-squirt *Styela clava*¹³. Other recently discovered seaweeds include three red algae—the *Grateloupia* mentioned above¹, another species of *Grateloupia*¹⁴, and a *Callophyllis* species (unpublished). These are currently under investigation; all could have originated from the eastern Pacific seaboard.

We would like to take this opportunity of endorsing the opinion of Dr Druehl that "the scientific community must assist in formulating reasonable regulations which will apply to the introduction of exotic marine species for scientific or economic purposes".

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Ufasomes are Stable Particles surrounded by Unsaturated Fatty Acid Membranes

THE arrangement of lipid molecules in biological membranes enables them to play both a structural part in providing a matrix for membrane proteins and a functional one, in which they act as a barrier to the free flow of solutes¹. These roles are usually fulfilled by phospholipids arranged in a flat bilayer with the hydrophobic segments of the molecules orientated towards the interior of the bilayer. The importance of this orientation is recognized in studies of model lipid membranes. Much of the knowledge of membrane lipid function has come from work with aqueous

dispersions of liposomes, or microscopic phospholipid particles, in which the molecular arrangement is similar to the orientation found in biological structures².

We have been interested in testing the hypothesis that membrane lipids may constitute a primary target in the initiation of radiation injury in higher organisms³. Part of the test requires analysis of chemical changes in irradiated membranes which may be correlated with alterations in function. Studies with intact membranes are hampered by their great chemical complexity and even liposomes made from natural phospholipids are too heterogeneous chemically for the ready unravelling of reactions which follow irradiation.

To overcome these difficulties we tried to form structures similar to phospholipid liposomes but made from pure long-chain fatty acids. Thin layers of various acids and acid mixtures were deposited in glass vessels by evaporation of chloroform solutions and the film resuspended by hand or mechanical shaking in water buffered at pH 8–9. The suspensions contained spherical particles visible under a microscope but showing no fine structure. The spheres were unstable to drying and the technique of negative staining for electron microscopy which works well for liposomes⁴ was unsuccessful. Freeze-etched specimens, however, showed clearly under an electron microscope that the spheres consist of an aqueous interior surrounded by one

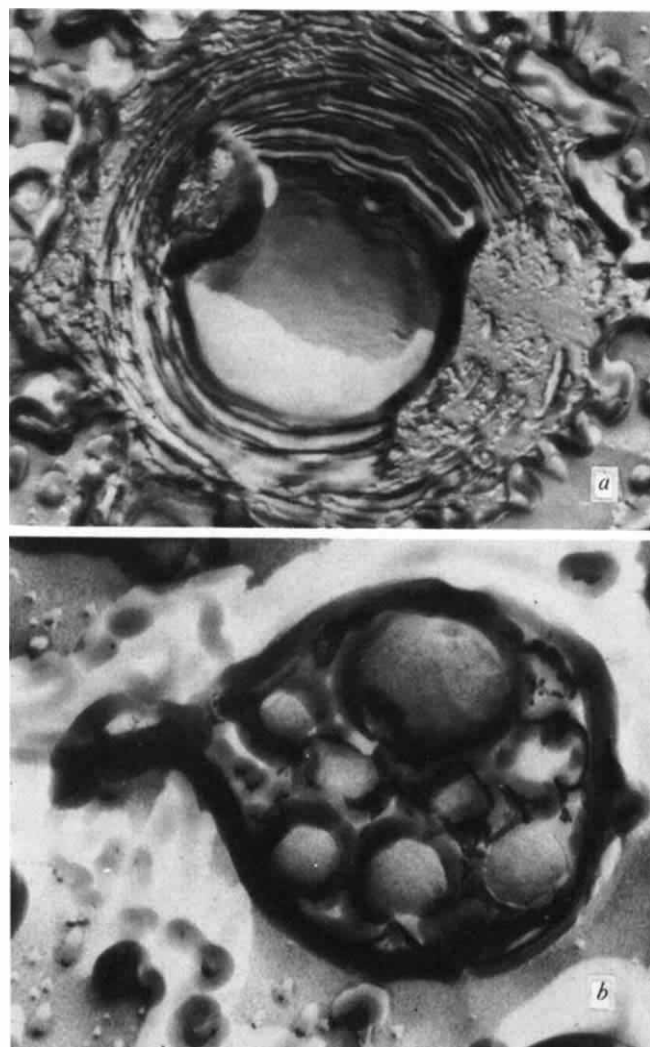


Fig. 1 Electron micrographs of freeze-etched oleic acid spheres ($\times 41,650$). *a*, A central aqueous region surrounded by concentric membranes—the flat pitted area was cut by the microtome. *b*, Smaller spheres enclosed by a common envelope.

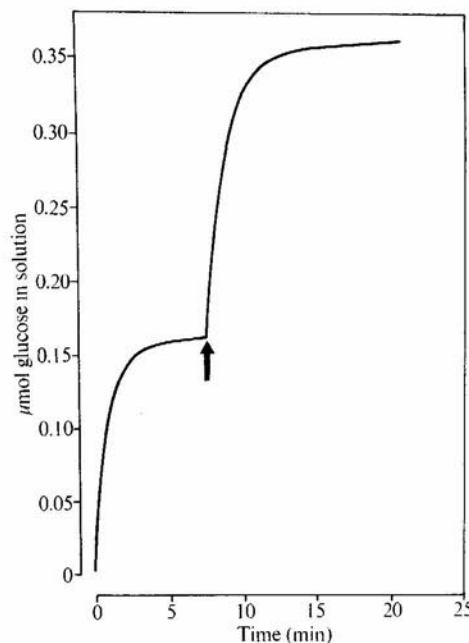


Fig. 2 Glucose retention by oleic acid spheres in 2.8 ml. of test solution (see text). The 0.17 μmol level corresponds to external glucose remaining after dialysis; the additional glucose was released when the spheres were disrupted with 'Triton' X-100, added at the arrow.

or more concentric layers of fatty acid membranes. A sphere made from oleic acid, shown in Fig. 1*a*, has at least twelve layers and resembles closely the electron micrograph of a phospholipid preparation published by Fluck *et al.*⁵. Some spheres have complex interior structures with many orientated sheets of incomplete membranes and others consist of collections of small spheres within a common envelope (Fig. 1*b*).

From the behaviour of fatty acids at interfaces⁶ it can be inferred that the membranes consist of double layers of molecules orientated with their hydrocarbon tails towards the interior of the membrane and the carboxylic groups in the membrane surface. The separation between individual bilayers varies between 20 and 30 nm (Fig. 1*a*). This suggests that here, as in liposomes, aqueous shells are interposed between individual membranes. The bilayers are stabilized by ionization of a small proportion of the weak acid groups at the slightly alkaline pH. Because of the obligatory presence of unsaturated fatty acids we have called the particles ufasomes.

The presence of aqueous spaces within the ufasomes was confirmed by forming them in the presence of glucose, removing exterior glucose by dialysis, and measuring the amount of sugar released by spheres disrupted with 'Triton' X-100 detergent. The spectrophotometer trace shown in Fig. 2 was obtained with ufasomes made from 2 mg of oleic acid shaken with 0.1 ml. of pH 8 Tris buffer and 0.1 ml. of 1 M glucose. After dialysis for 4 h, 0.05 ml. of the suspension was diluted into 2.8 ml. of buffered aqueous solution containing NADP, ATP Mg^{2+} , hexokinase, and glucose-6-phosphate dehydrogenase in amounts recommended by Demel *et al.*⁷.

The curve shows increase in optical density at 340 nm due to formation of NADPH in the presence of glucose. A correction was made for change in light scattering when the suspension cleared on addition of detergent. Clearly, the ufasomes held glucose in compartments inaccessible to the enzymes until their membranes were disrupted by detergent. Additional evidence for the presence of aqueous spaces in the particles was obtained from investigation of their osmotic properties, which will be reported elsewhere.

Although ufasomes are less directly related to membrane lipids than liposomes prepared from natural phospholipids, they offer the enormous advantage for model membrane studies of chemical purity and simplicity. Pure fatty acids are relatively cheap, whereas the nearest approach to phospholipids containing specific fatty acids is through synthetic materials, which are either expensive or require fairly tedious organic procedures⁸. Compared with liposomes, ufasomes emphasize the hydrophobic component of natural membranes. But the many parallels we have observed in the structure and behaviour of both types of particles suggest that it is those hydrophobic components that are dominant in determining the properties of the membranes.

An interesting feature of ufasomes is the possibility that they can form from molecules synthesized in the absence of living organisms. Speculations on the origin of life have to postulate eventually the formation of compartments surrounded by membranes in which restricted surface permeability can allow the development of complex synthetic mechanisms⁹. There is good evidence that mixtures of long-chain hydrocarbons were formed by inorganic processes during the Cambrian and older eras. Analysis of hydrocarbons from ancient rocks showed the constituents to have a typical chain length of C-20 or greater and to be highly unsaturated¹⁰. No fatty acids or porphyrins were present in these mixtures.

In contrast, younger deposits associated with organic marine sediments contain mainly normal alkanes, saturated branch-chain compounds and some porphyrins and fatty acids. In a sequence of events leading to the formation of unsaturated fatty acids, some Cambrian hydrocarbon mixtures could be brought to the surface through earth movements and subsequently carboxylated by atmospheric carbon oxides⁹. Thin films of fatty acids could form in evaporates and it would only require a subsequent wetting at slightly alkaline pH and some wave action or ground tremor for the simplest known membranous particles, the ufasomes, to form. The particles might already contain molecules synthesized through the action of radiation on the constituents of the primitive atmosphere. Formation of amino acids, purines, and porphyrins under such conditions has been confirmed¹¹⁻¹³. Alternatively, molecules such as NH₃, CO, CO₂, and CH₄ diffusing through the fatty acid membranes could condense within the particles in processes depending on a supply of external energy. Once formed, the more complex molecules would remain trapped for the lifetime of the particle. Other possibilities for synthesis of molecules essential for the appearance of life no doubt exist and are being actively studied in several laboratories. The feasibility of formation of ufasomes from simple prebiotic compounds may throw light on the mechanisms by which substances needed for appearance and development of living cells became concentrated and separated from their environment.

We thank Dr D. J. Goodchild and Mr N. A. Pyliotis of the CSIRO Division of Plant Industry in Canberra for the freeze etching of our specimens, and Mrs J. M. Gregory for assistance with the electron microscopy.

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Salicylates and Phospholipid Bilayer Membranes

FERREIRA¹ has observed that pain receptors are sensitized by prostaglandins, which strongly supports his suggestion that aspirin-like drugs induce analgesia by blocking the synthesis of prostaglandins. Salicylates, however, also produce an increase in the permeability of a molluscan neurone to potassium and a decrease in the permeability of the neurone to chloride which Barker and Levitan²⁻⁴ have suggested may be related to the analgesic action of the drugs. More extensive pharmacological investigations are obviously required to ascertain whether analgesia could be a membrane related phenomenon, and these studies would be facilitated by a knowledge of the molecular mechanism by which salicylates modify the permeability of neurones to electrolytes. A simple working hypothesis is that the "receptor site" of the neuronal membrane for salicylate is the ubiquitous⁵ phospholipid bilayer. An adsorption of salicylate anions to the bilayer would produce a negative electrostatic surface potential which would modify the interfacial ion concentrations and thus give rise to the observed changes in permeability. If the hypothesis is correct, salicylate should produce similar effects on neurones and artificial phospholipid bilayer membranes. It does.

Fig. 1 illustrates that salicylate increases the conductance due to the movement of a positive permeant species, the non-actin-K⁺ complex, through a bilayer membrane formed from a decane solution of the amphoteric but neutral lipid, phosphatidyl ethanolamine (PE). It has been shown that the antibiotic nonactin forms 1:1 complexes with potassium⁶, that it acts as a "neutral carrier" to transport the potassium ion across the membrane⁷ and that the conductance it produces may be used to assess changes in the electrostatic potential at the surface of the membrane⁸. Specifically,

$$G_{\text{Non-K}^+}^{\text{charged}} = G_{\text{Non-K}^+}^{\text{neutral}} \exp -(F\psi/RT)$$

where $G_{\text{Non-K}^+}^{\text{charged}}$ is the conductance of the charged membrane, $G_{\text{Non-K}^+}^{\text{neutral}}$ is the conductance of the neutral membrane in the presence of the same concentrations of potassium and nonactin and ψ is the potential difference between the surfaces of the charged and neutral membranes ($RT/F = 25$ mV at 22°C). The data in Fig. 1a imply that salicylate has adsorbed to the surface of the bilayer and produced the potential indicated by the right hand ordinate.

The increase in conductance observed in Fig. 1a is not due to the permeation of salicylate itself because the salicylate conductance is very low at neutral pH (Fig. 1b and ref. 9) and the membrane can be shown to be potassium permselective. Furthermore, it is not due to a change in some parameter unrelated to the electric potential, such as dielectric constant or viscosity, because salicylate would then increase the conductance due to a negative permeant species. Fig. 1b illustrates, however, that salicylate decreases the conductance due to a negative permeant species, the complex formed between the undissociated acid and anion of 5,6-dichloro-2-trifluoromethylbenzimidazole (DTFB¹⁰). In particular, 10⁻¹ M salicylate

increases the cation conductance 1.5 log units (Fig. 1a) and decreases the anion conductance 1.2 log units (Fig. 1b). The changes in conductance are thus approximately equal in magnitude and opposite in direction, confirming that the main effect of salicylate is to produce a negative potential at the surface of the bilayer membrane.

Both charges and dipoles could contribute to the change in potential, but as similar effects are observed at pH 5 and 7 (pK salicylate ≈ 3), the anionic rather than the undissociated form of salicylate must be responsible for the changes at neutral pH. As the theory of the diffuse double layer has been shown to describe adequately the potential due to charges at the surface of bilayer membranes^{8,13-16}, the Gouy equation may be used to estimate the charge densities required to produce the potentials illustrated in Fig. 1. These numbers agree qualitatively with direct equilibrium dialysis measurements¹⁷. It is therefore unnecessary to postulate that any substantial change in dipole potential occurs on adsorption of this particular molecule.

The degree of adsorption, moreover, does not appear to depend on the nature of the neutral lipid used to form the bilayer. When bilayers are formed from a 50% by weight mixture of cholesterol and phosphatidyl choline (PC) salicylate

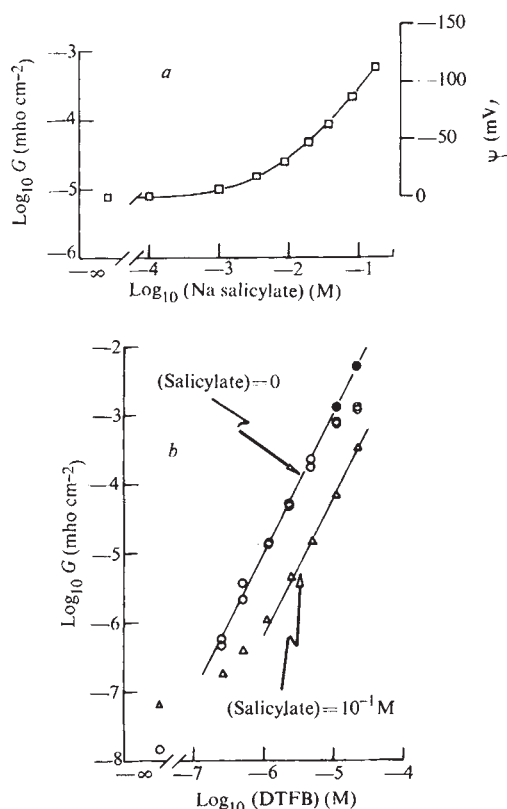


Fig. 1 *a*, Salicylate enhances the conductance of a cation selective bilayer membrane. The bilayer was formed from a decane solution of the neutral lipid PE in an aqueous solution of 10^{-1} M KCl, 10^{-6} M nonactin and either 2×10^{-8} M Tris or phosphate buffered to pH 7.0. The permeant species was the nonactin- K^+ complex. The right-hand ordinate illustrates the change in the electrostatic potential at the surface of the membrane calculated from the equation in the text. *b*, Salicylate depresses the conductance of an anion selective bilayer formed from PE. The bathing solutions contained 10^{-1} M KCl at pH 7.0 and the indicated concentration of sodium salicylate. The lines through the points have a slope of two, illustrating that the conductance, G , increases with the square of the DTFB concentration. This implies that the permeant species is a complex formed between the undissociated and anionic forms of DTFB, in agreement with results obtained with other substituted benzimidazoles¹¹. The results of two experiments ($\circ$) are shown in one case to illustrate typical scatter in the data; all other points are the average of at least two experiments. $\bullet$, Instantaneous measurements when these differ from the steady state values because of diffusion polarization in the aqueous unstirred layers¹².

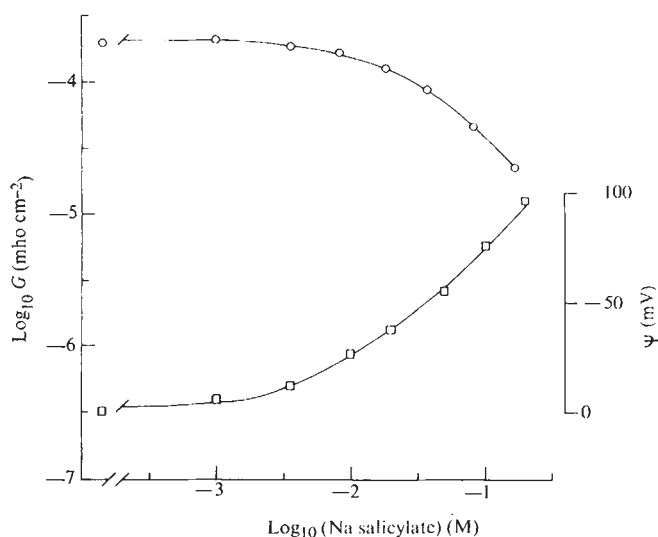


Fig. 2 Salicylate depresses the conductance due to a negative permeant species, $\circ$ — $\circ$, and enhances the conductance due to a positive permeant species, $\square$ — $\square$, when the bilayer is formed from a 1:1 mixture of PC and cholesterol in decane. The bathing solutions contain 10^{-1} M KCl, 2×10^{-3} M phosphate buffered to pH 7.0 and either 10^{-6} M nonactin ($\square$) or 6×10^{-6} M DTFB ($\circ$).

again increases the conductance due to a positive permeant species and decreases the conductance due to a negative permeant species (Fig. 2). Similar results have been obtained on bilayers formed from oxidized 7-dehydrocholesterol. The changes in conductance are approximately equal in magnitude, opposite in direction and identical with those observed in Fig. 1. It may be concluded, therefore, that the binding to these lipid bilayers involves primarily "hydrophobic" forces.

The data obtained on these artificial membranes agree qualitatively with the results obtained on neurones from the mollusc *Navanax inermis*. On the neurones² salicylate, at concentrations from 3–30 mM, increased the potassium conductance. On the bilayers, it produced a measurable increase in the potassium conductance at a concentration of 3 mM and an order of magnitude increase at 30 mM. Also, for a large number of salicylate and benzoate analogues there is a good correlation between the octanol-water partition coefficient of the undissociated acid and the ability of the analogue to change the membrane properties of the molluscan neurone¹⁸. These observations support the hypothesis that salicylates produce their effects on neurones and bilayers in a similar manner because the relative degree to which the neutral form of the analogue partitions into an organic phase should correlate with the relative degree to which the anionic form of the analogue adsorbs to a bilayer. Thus, 3,5-dichlorosalicylate has a calculated partition coefficient into octanol of 50 times that of salicylate¹⁸, is 30 times as potent as salicylate on a neurone¹⁸, and produces effects on the conductance of PE and PC bilayers similar to those illustrated in Figs. 1 and 2, but is 100 times as potent as salicylate. Benzoate, conversely, has a lower octanol-water partition coefficient than salicylate and is less effective than salicylate in enhancing the potassium conductance of either a molluscan neurone¹⁸ or a phospholipid bilayer.

A neutral analogue, salicylamide, also enhances the cation and depresses the anion conductance of neurones⁴ and bilayer membranes at comparable concentrations, the bilayer conductances changing about an order of magnitude when the drug is present at a concentration of 10 mM. Identical effects are observed on the bilayers at pH 5 and 7 (pK salicylamide ≈ 8.4) implying that the undissociated form of salicylamide is responsible for the changes in potential at neutral pH. The shapes of the curves obtained on bilayers are steeper than those illustrated in Figs. 1a and 2, as they should be, for this analogue must be changing the dipole^{15,19} rather than the double layer potential at the membrane solution interface.

The recent observation²⁰ that a salicylate analogue, 5-bromosalicylate, changes the relative cation permeabilities of the *Navanax* neurone would seem to argue against the hypothesis that these drugs modify only the surface potential of the neuronal membrane. The permeability ratios, however, were calculated from the Goldman equation^{21,22}, which implicitly assumes that the surface potential remains constant on addition of the salicylate. In view of the fact that 2 mM 5-bromosalicylate produces a change of about -60 mV in the surface potential of PE bilayer membranes, it would seem more appropriate to calculate the permeability ratios from Frankenhaeuser's²³ modified form of the Goldman equation, which explicitly incorporates the effects of a surface potential on the concentrations of ions at the membrane solution interface. This equation indicates that salicylates do not significantly alter the permeability properties of the cation selective "channels"²⁴ in *Navanax*. All the effects of salicylates observed to date on phospholipid bilayers and *Navanax* neurones can be accounted for in terms of a simple molecular mechanism; the hydrophobic adsorption of the salicylate to the insulating bilayer and the concomitant production of a negative surface potential.

Salicylates and benzoates, furthermore, are not unique among the weak acids in their ability to adsorb to bilayer membranes. The uncoupler 2,4-dinitrophenol²⁵ (DNP), the fluorescent probe 1-anilino-8-naphthalene sulphonate¹³ (ANS) and the indicator dye bromthymol blue (BTB) also adsorb: probably in much the same manner as salicylate because the shapes of the curves obtained with these molecules are all identical to that illustrated in Fig. 1a, but shifted about two, three, and four orders of magnitude to lower concentrations for DNP, ANS and BTB respectively. As "there is now substantial evidence that the major portion of the phospholipids is in bilayer form in a variety of intact membranes"²⁶, the anions of these and other amphipathic molecules should modify the surface potential of all essentially neutral biological membranes. Both squid axons²⁶ and frog myelinated nerves²⁷, however, seem to be negatively charged, illustrating the danger in extrapolating results obtained with amphipathic anions on either *Navanax* nerves or neutral bilayers to other systems.

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Synaptonemal Complexes in Brown Algae

THE small size of the nuclei of many brown algae makes it difficult to observe the chromosomes and their configurations in nuclear divisions, although life history studies and investigations of nuclei with the light microscope suggest that meiosis in these forms takes place in the unilocular sporangia. One means of testing the validity of this concept is to search for the presence of synaptonemal complexes because the relationship of meiotic chromosomes to these complexes has been well documented^{1,2}, and their presence has served as an indicator of meiosis in several members of the Myxomycetes^{3,4} and for several species of red algae⁵. Our search for such complexes has been successful, and we describe here their occurrence in two species, *Chorda tomentosa* (Laminariales) and *Pylaiella littoralis* (Ectocarpales).

We collected material of both genera in April 1972 at Gloucester, Massachusetts. *Chorda* was fixed in 3% glutaraldehyde, pH 7.2, post-fixed in buffered osmium and embedded in epoxy resin⁶. *Pylaiella* was fixed in 1% glutaraldehyde in seawater, post-fixed in osmium (also in seawater) and embedded in 'Epon'. Sections of both plants were cut on a Reichert OM U2 ultramicrotome with glass knives, stained with either 2% aqueous Ba(MnO₄)₂ or lead citrate and viewed with a Zeiss EM 9A electron microscope.

Young sporangia of *C. tomentosa* undergoing their first

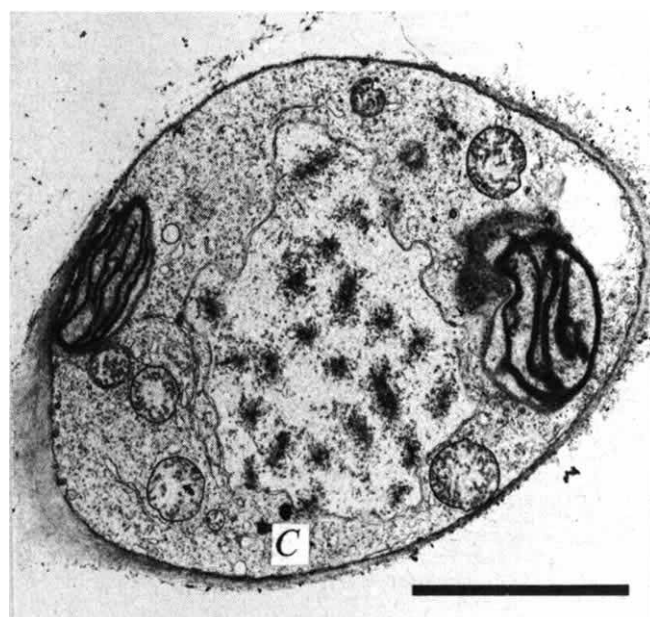


Fig. 1 Slightly oblique section of a young sporangium of *Chorda tomentosa* in prophase of meiosis. One of the two pairs of centrioles (C) can be seen near the nuclear envelope. Marker, 5 μ m.

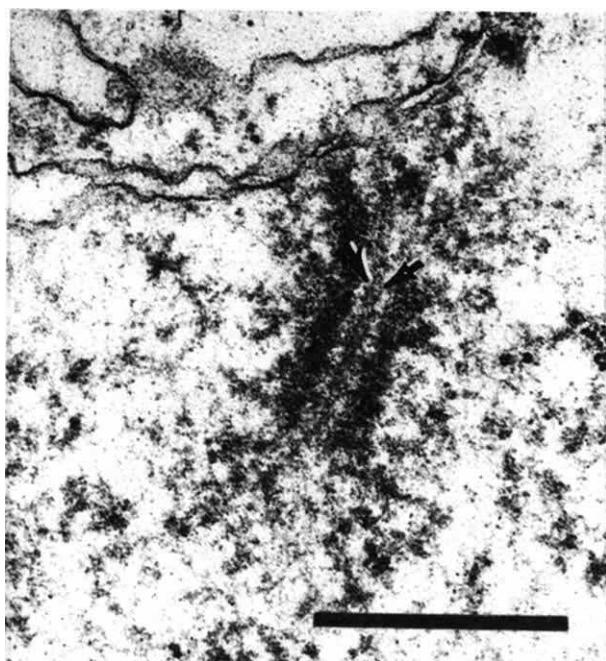


Fig. 2 Longitudinal section of a synaptonemal complex in *C. tomentosa*. The central element consists of two parallel bands (arrows). Marker, 0.5 μ m.

nuclear divisions were observed in prophase of meiosis (Fig. 1), with numerous synapsed chromosomes present within the nuclear envelope. The synaptonemal complex in *C. tomentosa* (Fig. 2) shows the characteristic structure of three parallel electron-dense elements separated by less dense regions. The two side elements vary in width from 60 to 110 nm, and are

separated by a 60–70 nm region containing the central element. The central element consists of two parallel narrow bands (arrows in Fig. 2).

Synaptonemal complexes in *Pylaiella littoralis* are found in nuclei of cells which are presumed to be developing unilocular sporangia. The synaptonemal complex consists of two side elements, 25–50 nm wide (arrows in Fig. 3), and a central element 25 nm wide (Fig. 3). The central space between the two side elements is approximately 50–75 nm wide. The synaptonemal complexes are close to the nuclear membranes (Fig. 3).

The discovery of synaptonemal complexes in *Chorda tomentosa* and *Pylaiella littoralis* helps to substantiate the concept that meiosis in the brown algae takes place in the unilocular sporangium. Serial sectioning and reconstruction using synaptonemal complexes^{5,7,8} may also yield accurate chromosome counts for many algal species where the small nuclear size has prevented accurate counts from being made⁹.

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Fig. 3 Longitudinal section of a synaptonemal complex of *Pylaiella littoralis*. Arrows indicate the side elements. Marker, 0.5 μ m.

Effect of Prohibition of the Use of Tetracyclines in Animal Feeds on Tetracycline Resistance of Faecal *E. coli* of Pigs

ALTHOUGH the use of tetracyclines in pig food in Britain was prohibited in March 1971 because of the accumulation of tetracycline-resistant *Escherichia coli* in the alimentary tract of pigs so fed, the veterinary use of tetracyclines continues. To assess the effect of this prohibition, I compared the incidence of these organisms in pigs brought to Chelmsford market in 1956, 1970 and July 1972.

On each occasion, thick aqueous suspensions of individual faecal specimens of 100 pigs, all of which had probably originated from different farms, were spread evenly over the surface of dried Petri plates of MacConkey agar and disks containing different antibiotics (Oxoid) were then applied at equal distances apart. The antibiotics were oxytetracycline (50 μ g), streptomycin (25 μ g), sulphonamides (300 μ g), chloramphenicol (50 μ g), ampicillin (25 μ g), polymixin (300 units), spectinomycin (25 μ g) and trimethoprim (1.25 μ g), the actual antibiotics used depending on their availability. The plates were incubated at 37° C and read. The method of recording was subject to some variation, but in general it was as follows.

All sensitive—when a broad zone containing no colonies resembling *E. coli* surrounded a disk. Nearly all sensitive—when a few colonies resembling *E. coli* were present in the inhibitory zone around a disk. Resistant and sensitive—when a disk was surrounded by a mainly confluent band of *E. coli*

growth, less profuse than the mass of the *E. coli* growth on the plate. All resistant—when growth of *E. coli* of uniform thickness took place up to the edge of a disk.

When difficulty was experienced in assessing sulphonamide and trimethiprim sensitivity, the relevant tests were repeated using DST Agar (Oxoid CM.261).

Table 1 The Antibiotic Sensitivity of the *Escherichia coli* in Faecal Specimens of Pigs Collected at Different Times from Chelmsford Market

Antibiotic	Year when specimens were collected	No. of 100 specimens in which the <i>E. coli</i> were:			
		All resistant	Resistant and sensitive	Nearly all sensitive	All sensitive
Oxytetracycline	1956	18	13	22	47
	1970	64	22	7	7
	1972	36	41	16	7
Streptomycin	1956	0	0	2	98
	1970	15	45	20	20
	1972	7	42	30	21
Sulphonamides	1956	2	2	4	92
	1970	12	32	18	38
	1972	5	41	29	25
Chloramphenicol	1956	0	0	0	100
	1970	0	3	2	95
	1972	0	1	3	96
Ampicillin	1970	1	20	15	64
	1972	0	12	18	70
Neomycin	1970	1	1	5	93
	1972	0	1	1	98
Furazolidone	1970	1	3	4	92
	1972	1	1	6	92
Polymixin	1970	0	0	0	100
	1972	0	0	0	100
Spectinomycin	1972	3	20	26	51
Trimethiprim	1972	0	0	0	100

The results are summarized in the table. Employing the method of Smith¹, the tetracycline resistance of 40 of 60 *E. coli* strains isolated from different faecal specimens in the 1970 and 1972 surveys was transmitted to a nalidixic acid-resistant mutant of *E. coli* K12F⁻. Of 30 streptomycin-resistant strains isolated from different pigs in the 1972 survey, 28 were resistant to sulphonamides and 16 to tetracyclines; the resistance of 13 of 28 of the strains resistant to streptomycin and sulphonamides was transmitted to the K12F⁻ strain.

It is probable that the substantial amount of tetracycline-resistant *E. coli* found in the 1956 survey was mainly the result of the first few years of use of tetracyclines as feed additives and the much greater amount in the 1970 survey the result of their continued use. If these results are typical of the position in Britain as a whole, then in 1970 the dominant *E. coli* organisms in the pig population were tetracycline-resistant ones, usually possessing conjugation ability and, as such, potentially able to transmit plasmids other than those coding for tetracycline-resistance. The 1972 survey suggests that during a 16 month period of prohibition of the use of tetracyclines as feed additives, the incidence of pigs harbouring tetracycline-resistant *E. coli* in their faeces had not decreased but that the amount of tetracycline-resistant *E. coli* in the pig population might have. The greater incidence of *E. coli* resistant to streptomycin and sulphonamides in the 1970 and 1972 surveys must be regarded mainly as the consequence of the therapeutic use of these drugs because they are not employed as feed additives for pigs.

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Supercontracting Striated Muscle in a Vertebrate

THE capacity of vertebrate striated muscle fibres to shorten is much curtailed by their Z-disks. Skeletal muscle sarcomeres, for example, can be reversibly contracted to about 40% of their fully extended length¹, an observation that accords well with the sliding filament model of muscle contraction^{2,3}. Once the thick filaments slide up against the Z-disks at the ends of the sarcomere, further shortening merely causes crumpling of the ends of the thick filaments¹ and a rapid decrease in tension. In marked contrast many unstriated muscles can exert tension whilst shortening to less than 10% of their fully extended length, as they have no Z-disks to interrupt the sliding of their filaments⁴. The regimenting and compartmentalizing influence of Z-disks in striated muscle supports speed and power of contraction but it is at the expense of shortening capacity.

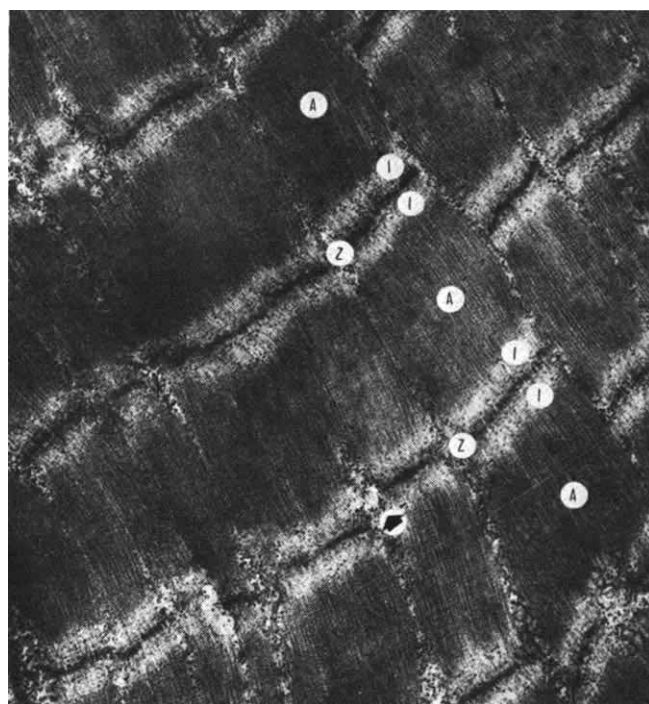


Fig. 1 Longitudinal section of chameleon tongue retractor striated muscle fibre $\times 13,200$. Z, Z-disks; A, A-zones; I, I-zones; arrow, glycogen granules.

In view of these well established properties of vertebrate striated muscle it was surprising to find striated muscle fibres in the tongue retractors of the chameleon (Fig. 1). This animal can project its tongue a considerable distance when attempting to capture prey and, in returning the extended tongue to the mouth, the retractor muscles shorten considerably and at high speed. The striated fibres concerned are longitudinally orientated, running from their origin on the hyoid to insert on the inner surface of the lingual bulb. Measurement of the resting and extended lengths of the tongue retractor muscle fibres showed a remarkable shortening capacity (Table 1).

Table 1 Shortening Capacity of Chameleon Tongue Muscles

Animal No.	Retracted length R (mm)	Extended length E (mm)	Ratio R:E
1	30	184	1:6.1
2	28	178	1:6.4
3	30	190	1:6.3
4	22	136	1:6.2

By electrically stimulating the retractor muscles it was found that they can powerfully and repeatedly contract down to about 16% of their extended length. This is a much greater shortening capacity than any previously described in vertebrate striated muscles and can be termed reversible supercontraction from analogy with certain invertebrate muscles⁵.

As it is the Z-disks that impose shortening limitations on most vertebrate striated muscles, these were examined for specialization. I decapitated chameleons, extended their tongues to about half their full length and prepared them for electron microscopy, taking care to maintain isosmotic conditions. At low magnification the tongue retractor muscle fibres showed the classical striation pattern, sarcomeres with A and I-zones and Z-disks being clearly demarcated; H-zones were sometimes evident although M-disks were not identified. Higher magnifications showed each sarcomere to have an almost conventional striated muscle arrangement, with thin filaments (diameter 5 nm) in the I-zone and thick filaments (diameter 20 nm) restricted to the A-zone. Regular perpendicular bridges, periodicity 35 nm, linked the two types of filaments together in the region of overlap; these resemble the cross-bridges found in vertebrate and insect muscle^{6,7}, although in the chameleon muscle the periodicity sometimes seems to extend into the I-zone and may represent troponin sites.

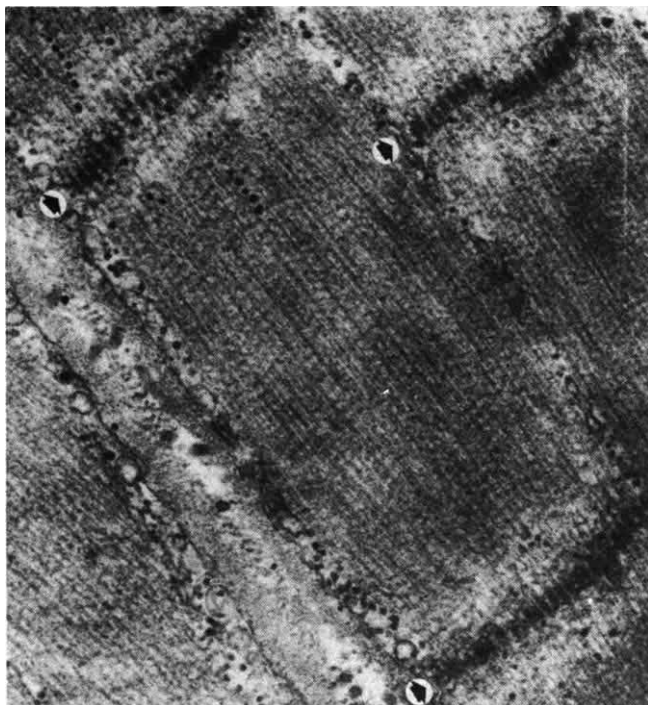


Fig. 2 Longitudinal section of chameleon tongue muscle sarcomere, $\times 36,080$. Arrows, perforated Z-disks.

The Z-disks have a very unusual appearance; instead of the normal continuous structures which completely separate the sarcomeres, they are discontinuous, presenting a fragmented appearance in longitudinal section (Figs. 2-3) and a shredded appearance in transverse section (Fig. 4). Three-dimensional reconstruction of the Z-disks of chameleon tongue muscle sarcomeres reveals that they are each divided into about twenty strips, each 30 nm wide; thin filaments from each side are attached to the strips in alternate pairs. The slits between the strips are open, enabling adjacent sarcomeres to communicate. The ratio of the numbers of thin filaments to thick filaments is approximately 4 to 1, much higher than in other vertebrate striated muscles.

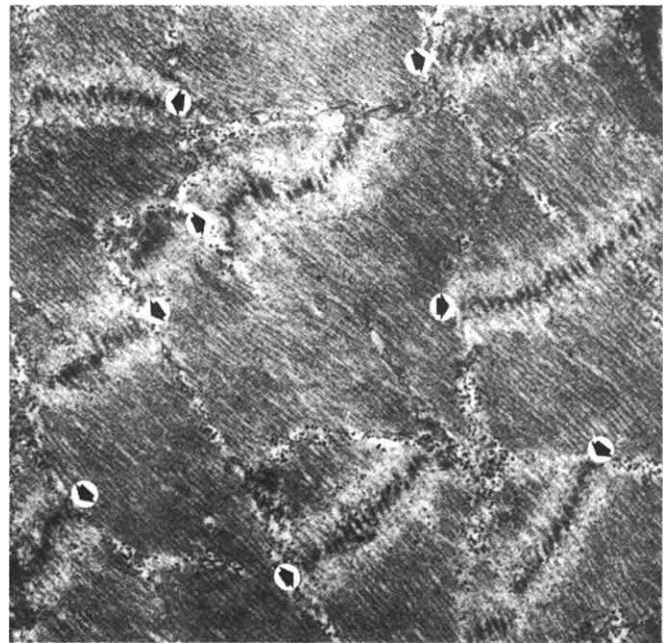


Fig. 3 Oblique section of chameleon tongue muscle, $\times 16,400$. Arrows, perforated Z-disks.

Supercontracting striated muscles occur widely among the arthropod invertebrates and were first described in a barnacle⁸. A second report concerned the insects—the body wall muscles of the blowfly maggot were found to supercontract⁹. Insect visceral muscles are all striated and many of them are of the supercontracting type¹⁰. As a result of these invertebrate studies (especially ref. 9), the process of reversible supercontraction is well understood in terms of the sliding filament model^{2,3}.

The mechanism of excessive shortening is based on the continuity of the sarcomeres by way of the perforations in their Z-disks. Contraction is not curtailed when thick filaments abut on Z-disks; instead the filaments slide through the holes into adjacent sarcomeres and shortening proceeds with little decrease in tension. By this process supercon-

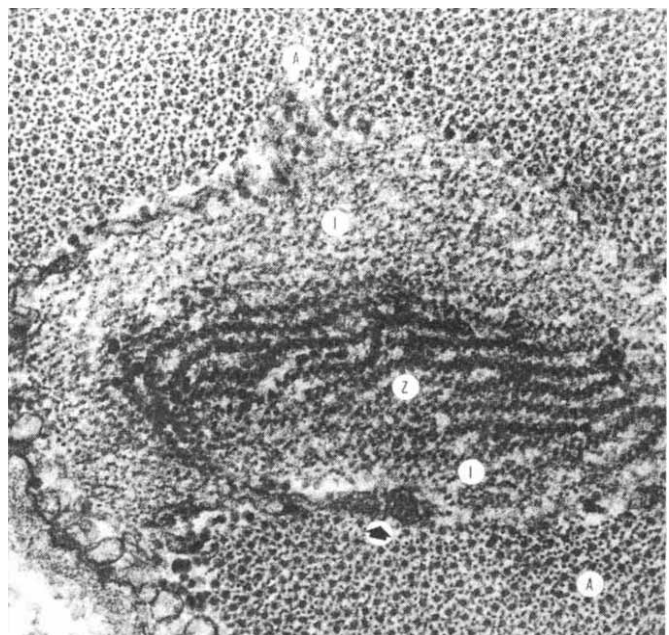


Fig. 4 Transverse section of chameleon tongue muscle, $\times 41,000$. Z, Z-disk; A, A-zones; I, I-zones; arrow, triad.

tracting muscles in arthropods combine the properties of striated and unstriated muscles¹⁰. In the chameleon, too, the striated tongue retractor muscle fibres have much of the force and speed of skeletal muscle (preliminary observations give a maximum tension greater than 1 kg cm⁻² and a maximum velocity greater than 1 m s⁻¹) in conjunction with a shortening capacity normally available only to unstriated muscle. The Z-disk perforations in chameleon muscles differ, however, from those of arthropod muscles, suggesting independent evolution of the supercontracting habit. A more detailed account of chameleon tongue muscles will be published elsewhere.

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Effect of Cigarette Smoking on Primary and Secondary Humoral Responses of Mice

THE bronchopulmonary system, one of the most important sites of access for antigen into the body, has a highly developed lymphatic system¹ and the capacity to effect a local immune response². Any modification of these defences by the environment is likely to have a lasting influence on the health of an animal. Chronic exposure to sulphur dioxide and carbon has been found to decrease the immune response of mice to inhaled antigen³. Cigarette smokers have also been found to have decreased serum antibody to influenza virus after infection or vaccination⁴. Both air pollution and cigarette smoking have been associated with an increased susceptibility to respiratory infection^{5,6}.

Antibody-producing cells have been found in the lungs of experimental animals after intratracheal (IT), intravenous (ref. 7 and our unpublished results) and intraperitoneal⁸ inoculation of sheep erythrocytes (SRBC). After IT inoculation antibody-producing cells have also been found in the spleen, blood and mediastinal lymph nodes (ref. 7 and our unpublished results). We have used the Cunningham modification of the Jerne plaque technique⁹ to locate direct (IgM) and indirect (IgG/IgA) plaque forming cells (p.f.c.) in smoked and unsmoked mice after IT inoculation of SRBC. Strictly line bred C57Bl mice were exposed to smoke for 26 weeks in a machine¹⁰ set to give a 1/7 smoke/air ratio at a puff volume of 35 ml. The animals were exposed to the smoke of thirty cigarettes daily (king size filter supplied by the Australian Tobacco Research Foundation). Animals were inoculated IT with 10⁸ SRBC in 10 µl of phosphate buffered saline (PBS) and the smoking regime continued. A second inoculation was given on day 12. Groups of 5 mice were examined for antibody producing cells at 7, 9, 12, 15, 18 and 21 d after the first inoculation. The animals

were exsanguinated and the blood leukocytes purified by dextran sedimentation. The cervical and mediastinal lymph nodes, spleen and lungs were removed, washed in PBS, minced, filtered through nylon wool and then washed and resuspended in MEM. About 10⁶, 10⁷ and 10⁸ white blood cells were recovered from the lung, lymph nodes and spleen respectively.

After IT inoculation the principal site of antibody synthesis in normal and smoked animals is the spleen, followed by the lymph nodes, with a small response in the lungs. The results are similar when considered per organ or per 10⁶ leukocytes. The only significant response found in the blood was in unsmoked mice 6 d after inoculation (5±2 direct p.f.c./10⁶ leukocytes). Responses in the spleen, lymph nodes and lung for smoked and unsmoked mice are shown in Fig. 1. All organs of the unsmoked, age matched controls developed a primary response, and after a second inoculation show an anamnestic response with increased indirect p.f.c.

Cigarette smoke modifies the immune response in all organs. The primary response in the lungs is virtually abrogated and

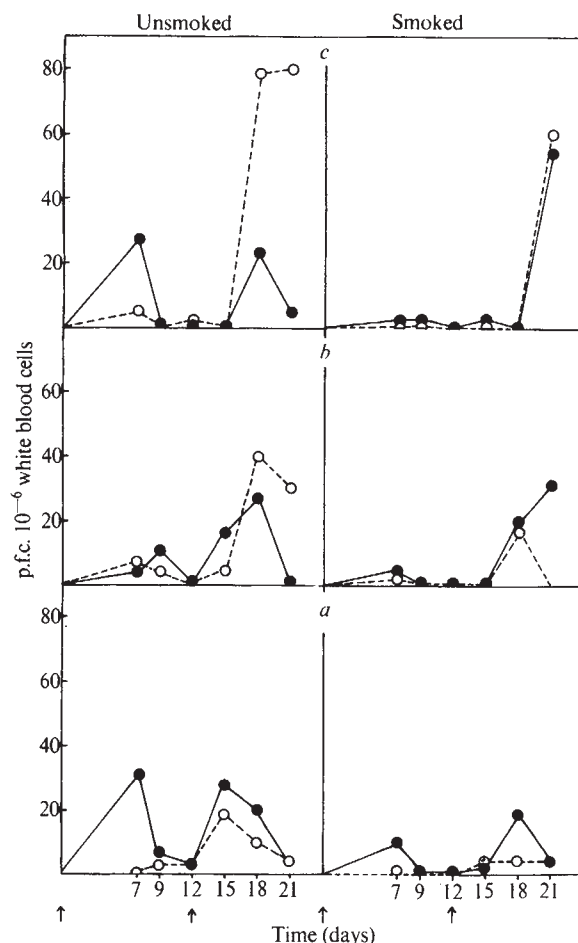


Fig. 1 Haemolytic plaque forming cells in (a) the spleen, (b) lymph nodes and (c) lungs of smoked and control mice after IT inoculation of SRBC. The smoked mice were exposed once daily (8 min) for 26 weeks in a Hamburg II small-animal smoking machine (Heinr. Borgwaldt, West Germany) set to deliver a 1:7 smoke: air mixture. All mice received a primary IT inoculation of 10⁸ SRBC on day 0, and a secondary IT challenge on day 12 (inoculation times indicated by arrows). Lung cells were suspended in Eagle's MEM at approximately 5×10⁶ ml⁻¹, spleen at 5×10⁷ ml⁻¹ and lymph nodes at 5×10⁷ ml⁻¹. 0.02 ml of 8% SRBC, 0.02 ml of guinea-pig complement and either 0.02 ml of PBS (for direct plaque) or 0.02 ml of a suitable dilution of anti-mouse globulin antiserum (for indirect plaques), was added to 0.02 ml of the cell suspension. The reaction mixtures were placed in Cunningham chambers in duplicate and sealed with wax. The plaques were counted after 30 min incubation at 37° C. ●—●, Direct p.f.c.; ○---○, indirect p.f.c.

although it is initiated in the spleen and lymph nodes it is reduced and short lived. After a second inoculation the response in the spleen of smoked animals was again reduced (particularly the indirect p.f.c.). The secondary response in the lungs is delayed longer than a normal primary response but resembles a secondary response in magnitude but with a greater participation of direct p.f.c. We found no acceleration of the secondary response in the lymph nodes, and although there is some increase in both direct and indirect p.f.c. the direct p.f.c. predominates.

The degree of reduction of the response seems to be related to the distance of the organ from the primary point of entry of the cigarette smoke. The immunosuppression recorded may well explain the increased susceptibility of smokers to respiratory infections. If immune surveillance¹¹ has a role in the elimination of cancer cells, then irrespective of any carcinogenic activity of cigarette smoke, damage to lung immunity may allow the escape of the cancer cell from a basic control mechanism.

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State-dependent Effects of Marihuana on Human Memory

THE abuse potential of marihuana has been widely debated as the use of the drug has increased in Western countries. Overton¹ has suggested that the abuse potential of various drugs may parallel their capacity for producing "state-dependent" effects in animals and man. State-dependent or "dissociated" learning refers to a phenomenon observed in animal and human studies wherein certain behaviour learned in particular drug states transfers more readily when the same drug state is reintroduced than if subsequent testing occurs under the influence of another drug or in a non-drug state²⁻⁸. This phenomenon has been demonstrated primarily with sedative and hypnotic drugs, such as barbiturates and alcohol, which are relatively susceptible to abuse. In general, drugs with little abuse potential (for example phenothiazines) dissociate weakly or not at all¹.

Barry and Kubena⁹ have shown that Δ^9 -tetrahydrocannabinol produces state-dependent effects in rats. In the study reported here we examined the effects of smoking

marihuana on various tasks, to see whether the drug produces state-dependency in man. If Overton's hypothesis is correct, detection of state-dependent effects from marihuana would be associated with its possible abuse potential.

Thirty-two men between the ages of 21 and 30 were paid to participate in a training session (day 1) and a test session (day 2) separated by 24 h. All subjects were screened for inclusion in the study using a structured medical and psychiatric interview. No subjects showed evidence of medical or psychiatric illness. All were experienced marihuana smokers; more than half had smoked during 4 or more years at least once a week. None had used marihuana or other drugs for at least 48 h before the experiment. The study was run single blind; that is, the subjects were not told that a placebo would be given. In most cases subjects given placebo were unaware they were receiving it. Because of the objective nature of the tasks, it was not necessary that experimenters be unaware of what subjects received.

Subjects were randomly assigned to one of four groups. One group (MM) received marihuana on both days. A second group (PP) received marihuana placebo on both days. A third group (MP) received marihuana on day 1 and placebo on day 2. A fourth group (PM) received placebo on day 1 and marihuana on day 2. This factorial design permitted analysis of drug, day and change-state effects. The change of state effect (*F* value) is reported in Table 1.

Table 1 Mean Errors and *F* Values for Change of State

Task	Day 2 - mean errors				<i>F</i> value
	PP	PM	MP	MM	
Ordered object recall	0.25	3.13	2.88	1.13	12.46*
Avoidance	14.25	13.88	15.38	10.88	0.64
Word association	0.50	2.50	1.25	3.13	0.01
Word strings					
Type <i>a</i>	0.69	1.75	1.32	0.94	1.00
Type <i>b</i>	2.19	3.07	2.19	2.37	0.15
Type <i>c</i>	1.94	2.75	2.62	2.00	0.63
Type <i>d</i>	3.06	4.82	6.19	3.94	5.78†
Types <i>a-d</i> (combined)	20.50	17.56	20.06	20.00	1.00

Data are based on analysis of four groups of eight subjects each. The first letter refers to the condition on day 1 and the second letter refers to the condition on day 2.

* *P* > 0.01.

† *P* > 0.05.

The marihuana contained 1.5% (–)- Δ^9 -tetrahydrocannabinol (THC) whereas the placebo consisted of marihuana with THC removed. Both drug and placebo were obtained from the National Institute of Mental Health. The marihuana was equivalent in strength to "street" marihuana imported into the United States from Mexico.

The procedure used in combusting and administering the smoke was similar to that used by Renault *et al.*¹⁰. A 9 l. spirometer (Collins Spirometer) with attached crucible was used. A 1 g sample of placebo or marihuana was burned in the crucible by pulling air through the material as the inner bell of the spirometer was raised, trapping the smoke in the bell. The crucible was then removed and replaced by a rubber mouthpiece. Subjects were instructed in the method of breathing to be used, consisting of inhalation for 5 s, holding the breath for 15 s, and resuming normal breathing for 35 s. This sequence was repeated until three canisters of smoke were inhaled; this required about 15 min. Testing began 15 min after smoking finished.

Tests included a visual avoidance task; a word association recall task; a verbal learning task measuring memory for "word strings" of varying degrees of meaningfulness, and a test measuring recall of ordered objects. Tests were given in a counter-balanced order during a period of 1 h. All tests except the visual avoidance task were designed to measure positive

transfer of learning from day 1 to day 2. The avoidance task measured interference or negative transfer. Where positive transfer is measured, dissociative learning is suggested when mean error scores in the same-state groups are lower than in the change-state groups. In the avoidance task, designed to measure negative transfer, dissociation would be reflected by fewer errors in the change-state groups than in the same-state groups.

In the avoidance task, four patterns of lights were randomly presented. Each pattern could be extinguished by a specific switch controlled by right and left hands and feet. An incorrect response or failure to respond within 3 s resulted in presentation of a noxious tone. The criterion was twenty correct responses, with number of errors to reach criterion taken as the measure of performance. The task was identical on both days, except that on day 2 the pattern-switch relation was altered. Thus, performance on day 2 was assumed to reflect interference; that is, the greater the number of errors on day 2, the greater the degree of interference.

The rote learning task involved memorizing four five-word "word strings" of varying meaningfulness (normal sentence, anomalous sentence, anagram and word list)¹¹. On day 2 subjects were asked to recall the word strings memorized on day 1 after which a relearning session was conducted. The following word strings were used. (a) The normal sentence was: "Soapy detergents dissolve greasy stains"; (b) the anomalous sentence was: "Pink accidents cause gallant storms"; (c) the anagram was: "Neighbours sleeping noisy wake parties"; (d) the nonsense word list was: "give odours snows fatal violent". Performance was measured in terms of errors of sequence and omission.

For the word association test, twenty words of low association value¹² were presented. Subjects were instructed to respond to the stimulus words with the first word that came to mind. On day 2 the stimulus words were repeated and subjects were asked to recall their responses on day 1. Performance was measured in terms of errors made in day 2 recall.

In the object recall test subjects were shown seven plastic objects from the Kahn test of symbol arrangement¹³. They were asked to name each so that subsequent deficits in recall presumably would reflect memory loss rather than inattention. The subjects were then asked to order the objects from left to right. On day 2 subjects were asked to recall the seven objects and then arrange them in the same order as on day 1.

All measures of performance were subjected to 2 by 2 factorial analyses of variance. Table 1 presents the interaction *F* values for the four tasks and the mean errors for each group. The interaction effect tests the marihuana state change effect. Thus, any significant interaction may be taken as evidence of dissociation.

As Table 1 shows, the recall of ordered objects showed a significant interaction effect ($P < 0.01$). The dissociative effect arose from the MP and the PM groups making more errors than either the PP or MM groups. Analysis of simple principal effects indicated that both the PM and MP groups differed significantly from the PP group ($F = 9.63$, $P < 0.01$; and $F = 8.03$, $P < 0.01$, respectively). Also, the PM group differed significantly from the MM group ($F = 4.66$, $P < 0.05$).

Neither the avoidance task nor the word association task revealed a significant interaction effect. In the word string task, with errors analysed by type of string, there was evidence of dissociation for the type *d* string (Table 1) in which both syntax and meaningfulness are missing. The other three types of word strings provided these cues. The normal sentence provided both syntax and meaningfulness, while the anomalous sentence provided syntax, and the anagram provided meaningfulness.

Although an interaction effect was not found in the word association task, a significant drug effect was evident ($F = 8.73$, $P < 0.01$). Thus, marihuana disrupts memory for self-generated word associations, though this effect is no more pronounced in the change-state groups than in the same-state groups.

The significant results found in the ordered object recall and

the type *d* word string (Table 1) suggest that tasks involving sequential memory may be most sensitive to dissociation. The lack of dissociation found for the *a*, *b*, and *c* type word strings may be explained by the fact that recall of the sequence is aided by long term memory for sentence structure or meaningfulness of words. In contrast, the type *d* string must be recalled sequentially without the aid of either. This task seems to have much in common with sequential recall of the plastic Kahn objects, where meaningfulness also plays an insignificant role.

The results of the present study suggest that marihuana produces "state-dependent" learning when tasks sensitive to dissociation are used. In other studies of state-dependent learning, using other drugs, similar differential effects were found, depending on the task and the drug administered. Other investigators have reported disruptive effects of marihuana on tasks involving sequential adjustments in reaching a goal¹⁴.

If the abuse potential of drugs parallels their capacity to produce state-dependent or dissociated learning, as Overton¹ has suggested, then one must conclude that marihuana has an abuse potential. However, the degree of dissociation obviously varies as a function of task variables in addition to any correlation it may show with abuse potential.

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Survival of Common Bacteria in Liquid Culture under Carbon Dioxide at High Temperatures

We report results which are at variance with the general belief that high temperatures are necessarily lethal to microorganisms¹. *Escherichia coli* B, *Aerobacter aerogenes*, *Bacillus subtilis*, and *Serratia marcescens* were found to survive for 24 h in a CO₂ atmosphere at a temperature of up to 160° C. These organisms were routinely cultured in sterile trypticase soy broth (TSB) medium, and plated on TSB plates containing 1.5% of agar.

Table 1 Effect of Temperature and High Pressure CO₂ on the Survival of *E. coli*, *A. aerogenes*, *S. marcescens*, and *B. subtilis*

Expt	pCO ₂ * Total (atm)	pressure †	Time (h)	Temp. (°C)	Control Final	Viable counts‡ Exposed	Survival (%)
<i>E. coli</i>							
1	10.1	11.2	24	105	3.6	7.3×10^5	2.0×10^{-3}
2	10.1	11.2	19	110	8.6	3.1	36
3	11.3	12.7	24	110	4.2	1.0×10^{-4}	2.4×10^{-3}
4	9.5	10.9	24	110	0.5	1.0×10^{-3}	0.2
5	9.5	10.9	24	110	3.4	2.5×10^{-3}	7.0×10^{-2}
6	9.5	10.9	24	110	3.7	1.9×10^{-5}	5.0×10^{-4}
7	9.5	11.8	24	125	4.0	4.0×10^{-4}	10^{-2}
8	9.5	11.8	24	125	3.4	2.0×10^{-5}	6.0×10^{-4}
9	9.5	11.8	24	125	3.8	1.3×10^{-3}	4.0×10^{-2}
10	9.5	11.8	24	125	2.7	2.2	81
11	17.0	20.3	20	125	11.5	3.0	26
<i>A. aerogenes</i>							
12	9.5	11.8	24	125	5.4	0.25	4.7
13	9.5	11.8	24	125	5.4	0.27	5.0
14	19.7	25.8	24	160	1.3	5.1×10^{-4}	4×10^{-3}
15	19.7	25.8	24	160	0.36	2.9×10^{-4}	8×10^{-2}
16	19.7	25.8	24	160	0.27	10^{-6}	4×10^{-4}
17	19.7	25.8	24	160	0.29	8.0×10^{-5}	3×10^{-2}
18	19.7	25.8	24	160	0.34	0	0
19	19.7	25.8	24	160	0.31	4.3×10^{-3}	1.4
20	19.7	25.8	24	160	0.32	5.0×10^{-5}	1.6×10^{-2}
<i>S. marcescens</i>							
21	10.1	11.2	19	105	1.6	0.02	1.2
22	11.3	12.7	24	110	5.3	1.1×10^{-4}	2.1×10^{-3}
23	11.3	12.7	24	110	6.9	1.3×10^{-4}	2.0×10^{-3}
24	9.5	11.8	24	125	5.2	1.1×10^{-3}	2.0×10^{-3}
25	18.0	22.9	21	152	2.8	6×10^{-6}	2.1×10^{-4}
26	19.7	25.8	4.5	160	14.0	5.0	36
<i>B. subtilis</i>							
26	9.5	11.8	24	125	3.3	0.21	6.4
27	19.7	25.8	4.5	160	4.2	5.4×10^{-6}	1.3×10^{-4}

* Given at the experimental temperature. † Including water vapour pressure, at the experimental temperature¹³. ‡ Figures have been multiplied by 10⁸, and represent cells per ml. given as exposed/control counts (viable, after 24 h at 20° C), expressed as a percentage.

Cultures were incubated at 37° C on a rotary shaker, subcultured every 48 h, and exposed to high temperatures and pressures while in the stationary phase, 48 h after subculturing. One millilitre of each culture was injected into 5 ml. glass tubes, which were then inserted into 75 m stainless steel bombs fitted with a plug and a needle valve, and flushed free of air with commercial grade CO₂ for 30 s before sealing and raising the pressure to up to 20 atm, and enclosing the entire bomb in an oven at 105–160° C for up to 24 h. Pyrene (m.p. 150° C) was liquefied inside such a bomb within 1 h at an oven temperature reading of 150° C, demonstrating effective heat transfer. Control tubes of each culture were maintained at 20° C in air during the experiment, to provide data on viability loss through standing.

After exposure, dilutions of 10¹–10⁸ were made of test and control cultures, by transferring 1 ml. of each into 9 ml. of sterile TSB in screwcapped tubes, shaking, and repeating the transfer eight times, using a new 1 ml. disposable pipette for each transfer. Each dilution was then plated in triplicate and incubated at 37° C for 7 days, when viable counts were obtained.

As checks on possible contamination, we used (1) microscopic examination of exposed cultures, (2) a procedural blank of 105 plates obtained from a sample of medium exposed for 24 h at 105° C and 9.5 atm CO₂ inside a bomb, and (3) the Enterotube diagnostic method². The control experiment was performed throughout as though a live culture were being used, and yielded four plates contaminated with fifteen colonies, which were easily distinguished from our test organisms, and which appeared after incubation for 6–7 days on the top plates of the stacks. The Enterotube test (Roche Diagnostics, Nutley, NJ) consisted of the inoculation of colonies obtained on plates from previously heated and pressurized cultures, into the

Enterotube, which was then incubated at 37° C for 24 h. These tubes give colour changes which are diagnostic for individual species. In fourteen of fifteen colonies of *E. coli* and *S. marcescens* which were thus tested after exposure to high temperature and pressure, the result was positive for the test organism.

The results obtained are summarized in Table 1. There were, however, six experiments, of which three were with *E. coli*, two with *B. subtilis* and one with *A. aerogenes*, in which no survival was recorded. The percentage survival varied from 0 to 81.

The medium, after exposure, was clear yellow with no sign of charring. Cells were mobile when viewed under the microscope, although some clumping was noted. The motion was not Brownian motion.

The composition of the gas phase inside the bombs was not constant during an experiment, or between experiments, as different corrections had to be applied for the saturation vapour pressure of water. At 160° C this correction is 6.1 atm. Simple calculation shows that the water loss never exceeds 18% of the total volume. The cultures are therefore never dry. The carbon dioxide gas used was analysed by mass spectrometry and had the following composition: H₂O (11 p.p.m.), Ar (1), CH₄ (1), N₂ (24), CO (<1) and O₂ (225).

These results cannot be explained as contamination. Our controls from the Enterotube results were positive. Further *S. marcescens* yielded red colonies after exposure to high temperatures and pressures. As we are unable to detect a false step in our experimental procedure which could have led to these results, we believe the data are real. Common micro-organisms thus appear to be able to survive long periods at temperatures normally considered lethal, if carbon dioxide is present. These results may have some implications for the exploration of the atmosphere of Venus by the use of space probes. Sterilization of space probes may be necessary.

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Visibility of Vehicle Rear Lights in Fog

WHILE driving in daylight fog vehicle rear lights often become lost to view at about the same distance as the vehicle carrying them. The brighter the light, the further will it be visible through fog^{1,2}. That the area of the light itself is also important is less well known.

A ray of image-forming light, in its passage from an object to the eye, is reduced in intensity by absorption and by light being scattered out of its path by the water droplets in the fog. There is, simultaneously, a gain in brightness as a result of non-image forming diffuse light producing a veil of brightness between the eye and the object. Thus the apparent luminance of both the vehicle and its lights tends to equal that of the background fog. As the viewing distance is

increased the contrast between the vehicle and its lights diminishes to a level that the eye cannot detect.

Contrast can be defined³ as $(O-B)/B$ where O is the object radiance and B is the radiance of its background. For monochromatic light Duntley⁴, using Koshmieder's equation⁵ for radiance transfer through turbid media, has shown that the apparent contrast between an object and its background decreases exponentially with distance. If O is a light source, the brighter the light the further away will it be visible. Accurate calculations must take into account the sensitivity of the eye, the radiance distribution of O and B and the spectral transmission of the intervening medium. For most field work on visibility, however, these factors, including the red colour of the light, are not very significant⁶.

The intensity of the ambient light and the angle subtended by the object at the eye are the principal factors affecting the ability of the eye to detect contrasts³. In daylight fog the intensity of the ambient light is high, but so is the veiling light. Thus visual contrasts tend to be low, but the contrast perception threshold is also low. Above about 1° the visual angle is not very significant, but for smaller angles the contrast perception threshold of the eye increases rapidly. A light 6 cm in diameter subtends a 1° angle at 3.4 m so in most traffic situations the angular subtense of the rear light is significant.

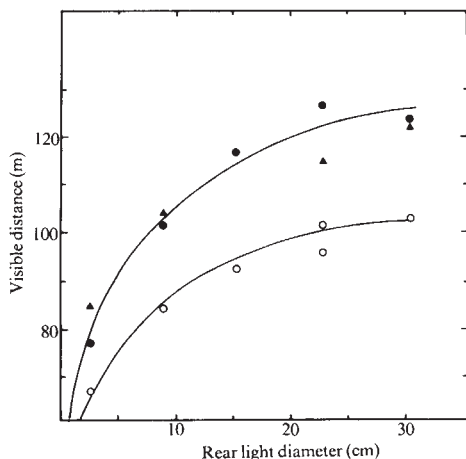


Fig. 1 Distance that red lights of various diameters can just be distinguished through fog by the two observers, J. N. L. (○, ●) and E. R. C. (▲). ○, 1000–1030 h on January 23, 1973; ●, ▲, 1040–1105 h on January 25, 1973.

Fig. 1 shows the visible ranges of red lights of various diameters in hill fog on Ditchling Beacon, Sussex, England. A uniform red light 30.5 cm in diameter was obtained by mounting a Fresnel filter and a red acetate filter in front of a 48 W tungsten headlight 18 cm in diameter. The light was diffused and dispersed by two sheets of clear glass between the lamp and the lens. The first sheet had a stippled pattern, the second a diamond pattern. The diamond glass was in the plane of focus of the lens. Lights of different diameter were obtained by a series of black masks.

The light had an intensity of 3.5 lm cm^{-2} , which is roughly equivalent to an average brake indicator light or one of the less bright purpose-made accessory rear fog lamps². The light was mounted in the rear of a red 22 cwt Bedford van. A cord was extended from the rear of the van away from the light. The observers walked back along the cord until the point at which the light could just be distinguished. The position on the cord was then marked with tape. Immediately afterwards a second measurement was taken for the visible range of the black surround to the light. There was no measurable difference between the visibility of the black surround and the red van.

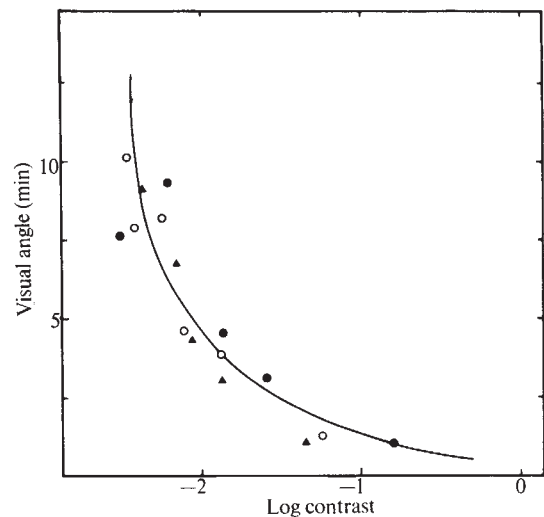


Fig. 2 Relationship between the visual angle (arc min) subtended by the test lights, and the calculated brightness contrast at which they are just perceptible. The points correspond to those in Fig. 1. By comparison, the curve is drawn from Blackwell's data³ for the contrast perception threshold for different visual angles in good light (1,000 foot L).

There was no overall change in the density of the fog during the course of the experiments, although on both occasions a second experimental series had to be abandoned when the fog began to lift rapidly. The estimates for the visible range of the vehicle were more difficult to make, but were 82 ± 2 m for J. N. L. on January 23, and 95 ± 4 m for E. R. C. and 92 ± 3 m for J. N. L. on January 25. Thus, with reference to Fig. 1, it can be seen that a rear light smaller in diameter than 7.5 cm will be visible at a shorter distance than the vehicle carrying it and that within the range of the experiment the larger the rear light the further it will be seen.

The increase in visible range with rear light diameter can indeed be explained in terms of the angle it subtends at the eye (Fig. 2). The reduction in brightness contrast with distance can be calculated from Duntley's equations⁴, for the brightness contrast presented by a black object near to the eye is -1 and the contrast perception threshold in good light is near 0.004. By comparing the visible range of the largest of the lights with that of the black surround and making appropriate provision for the angular subtense of both, it is possible to calculate the brightness contrast of the lights at their maximum visible ranges in terms of the angle they subtend at the eye. It is clear that these results compare well with those to be predicted from Blackwell's data.

In practical terms it would seem that most vehicle rear lights are too small and are indeed of very little use in thick fog in daylight. An increase in diameter up to about 20 cm would, however, result in an increase of 75–85% in the visible range that one vehicle can be seen from another.

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⁴ Duntley, S. Q., *J. Opt. Soc. Amer.*, **38**, 179 (1948).

⁵ Middleton, W. E. K., *Vision Through the Atmosphere* (University of Toronto Press, 1952).

⁶ Middleton, W. E. K., *Visibility in Meteorology* (University of Toronto Press, 1941).

BOOK REVIEWS

Piagetian Theory

Psychology and Epistemology: Towards a Theory of Knowledge. By Jean Piaget. Translated from the French by P. A. Wells. Pp. vii+107. (Allen Lane, Penguin Press: London, 1973.) £2.50.

IN his novel *Juan in America*, Eric Linklater remarks that the inhabitants of the American Middle West, though speaking English, think German thoughts. Piaget, on the contrary, though writing French, thinks English thoughts. This is nowhere more manifest than in this new collection of six essays from recent Piagetian writings.

The theme of the collection is the relationship between psychology and philosophy, though there are occasional remarks about the relationship between the former and other academic disciplines such as sociology and economics: but the main burden of argument turns on possible relationships between developmental psychology and epistemology and logic in particular.

It is Piaget's view that the concrete studies of psychologists, particularly developmental psychologists, can provide a kind of fundamental groundwork of hints for the conceptual analyses of operations and concepts which are the province of philosophers proper. The upshot is a kind of synthesis of what one might call the "Cambridge" and the "Oxford" ways. The Cambridge way looks to the formal analysis of concepts as the method of philosophy, while it has traditionally been the Oxford style to base all conceptual analyses in a set of very concrete exemplars. This perhaps is a way of representing the difference between the formalist and ordinary language schools of analysis in contemporary philosophy. In his inimitable way, Piaget succeeds in reaching a point of synthesis between these two main contemporary trends.

The synthesis is achieved by paying attention to the developmental history of a conceptual or logical operation. For example, Piaget shows in the case of number how some kind of synthesis between the Russellian conception of number as a set of sets, and the Peano conception of number as a simple ordering of objects according to a certain scheme, can be brought into some kind

of relationship. Developmental psychology shows that a child first has to acquire the idea of a set from the notion of simple ordering. He will conceive of a set in the first instance only if it is capable of an organization of the Peano conception. Once the notion of set has thus been grasped, the child is capable of setting one set in one-one correspondence to another similarly derived. Finally, by an act of abstraction, the child comes to grasp that the order of the elements in the set is of no numerical consequence and that all that matters numerically is the one-one correspondence between like sets. In this way Piaget, in this and many other examples, sees the possibility of a synthesis between the formal and the concrete.

There are all sorts of other ideas developed in more or less detail in the book. Perhaps the most significant for the contemporary scene are his ideas about the possibility of inter-disciplinary studies. He envisages a very large number of combinations between sociology, linguistics, psychology, new forms of abstract engineering such as cybernetics and information theory, all related in one way or another to and through philosophical analysis. It is only at this point that one feels that Piaget's isolation within an essentially French-speaking world has led to him being somewhat surprisingly out of touch, though of course this may be due to the fact that these essays were written some years ago. He seems not to be aware that the project which he sketches of a thoroughgoing interaction between psychology, sociology and philosophy is already in full flood in what has come to be called "the ethogenic movement", derived from many sources, some of which, such as phenomenology, one would expect Piaget to be fully familiar with. One might say that he has identified the place where development can take place but has apparently not yet discovered that there is where the action is.

In one other and striking way these essays seem a little out of touch. They embody a curiously naive "laws in deductive structure" view of science, though Piaget is quick to repudiate an identification of his method with that of any form of logical positivism. In

this way he shares the assumptions of many of his generation as to the proper analysis of science, while rejecting the logical positivism from which it ultimately derived. This is matched by an apparent lack of awareness of the rapid contemporary decline in confidence in both experimental psychology and formal methods of philosophical analysis, to both of which Piaget remains devoted. And yet it is clear that the spirit of the rapprochement between epistemology and developmental psychology that he advocates points strongly in the direction of the new and radical ideas as to what empirical psychology and philosophical analysis ought to be. In short, this book is as interesting and provocative as Piaget's more major works. One ought to remark finally on the excellence of the translation. As I said earlier in this review, Piaget's style of thought is, I think, more Anglo-Saxon than French, and perhaps this leads one to feel that in translation the book reads as if it were written directly into English.

ROM HARRÉ

Fourier Probabilists

Fourier Analysis in Probability Theory. By Tatsuo Kawata. Pp. xii+668. (Academic Press: New York and London, November 1972.) \$32.

FOR many years now the theory of Fourier series and integrals has been a powerful tool for studies in the mathematical theory of probability. But as classical Fourier analysis has retired from the front of the mathematical stage, many probabilists find themselves ignorant of the possible refinements of the tool, and their mathematical armoury is correspondingly weaker. If this book succeeds in reminding practitioners of probability theory of the deep and powerful results of old-fashioned hard Fourier analysis, it will have performed a useful service.

Most of the book is a workmanlike account of the theory of the Fourier transform and its relatives in the classes L_1 and L_2 . Although this treatment is written with applications to probability theory in mind, it includes many results

which have not yet found such applications. Even the experienced reader may find himself in deeper water than he expects, and familiar theorems take fresh meaning from unfamiliar contexts and formulations.

The latter part is concerned with some areas of probability theory in which Fourier analysis plays a part. Pride of place naturally belongs to the continuity and compactness theory of characteristic functions, but the author devotes a good number of pages to topics related to the convergence of random series of different types. It might be argued that he could usefully have given a wider and more superficial survey. For example, the early parts of the book contain the prerequisites for the arithmetical theory of probability distributions (which has recently come into prominence again in the context of "delphic semigroups"). Again, there is no sign in this book of the very extensive work carried out in recent years, mainly in Eastern Europe, on the rate of convergence in central limit theorems and the theory of large deviations. Moreover, the Fourier theory is entirely classical, and there is for example no recognition of the importance of that elusive and challenging object, the convolution algebra of measures on the line.

These criticisms of course merely say that the author has not written a different book. The one he has chosen to write will prove most useful to any probabilist who wants to be able to use the tools of hard analysis, and who is not content to work on easy problems made to look significant by perverted abstraction.

J. F. C. KINGMAN

Immunotechnology

Methodology of Immunochemical and Immunological Research. By J. B. G. Kwapinski. Pp. xx+820. (Wiley: New York and London, January 1973.) £16.60.

To the outsider the various branches of immunology could seem equivalent to the sects of a religion; the differences of observance can obscure the common theme. The pioneer immunologists were often medical microbiologists interested in the notion of immunity as a defence mechanism against invasion by microorganisms. Their efforts in the past have been remarkably successful in eradicating a number of previously common and serious diseases. In more recent years two further major trends in immunology have emerged. First, the nature of immunoglobulin antibody molecules has been delineated; and second, the origins of the immunologically responding cell populations have been revealed. The older formulation of immunology and the newer disciplines

have each developed their own technologies. In this book by Professor Kwapinski, which is a more up to date version of his *Methods in Serological Research* (1965), it is elaboration of the older formulation with which he is principally concerned. An author should not necessarily be taken to task for the information given on the cover of his book but in this instance it must be pointed out that the title and the cover are misleading in that it is only a restricted view of immunology which is presented. For example there are two large chapters on antigens and their characterization which include nothing on histocompatibility antigens; the emphasis is almost entirely on bacterial and viral products. Thus the book is something of an anachronism but it may nevertheless be of value when the roots of immunological research are re-explored as they surely will be in the not too distant future. The illustrations are of poor standard and doubtful usefulness and the price is very high if the book is to serve as the "ideal text for students in medicine, dentistry, science and biological sciences"—to quote the cover.

A. J. S. DAVIES

Behavioural Biology

Essentials of Physiological Psychology. By S. P. Grossman. Pp. vii+506. (Wiley: New York and London, 1973.) £6.65.

In 1967 the author published *A Textbook of Physiological Psychology*, of 932 pages, and intended for graduates. Now five years later he has produced *Essentials of Physiological Psychology*, of 506 pages, and intended for an introductory course. The layout of this book is like that of the larger. It is divided into four main sections. The first (three chapters) is an introduction to neurophysiology and neuroanatomy. The second (five chapters) covers some of the senses, vision, audition and the chemical senses, together with chapters on the motor system and the reticular formation. The third section has seven chapters on motivation and emotion, dealing with hunger, thirst, sexual behaviour, emotional behaviour, reward and punishment, sleep and arousal, and finally with theories of motivation. The last section (five chapters) reviews work on the localization of capacities for learning, and the electrophysiology and biochemistry of learning. This book differs from the previous one in several respects other than its reduced length. It contains a chapter on research methods in the first section which briefly describes techniques of ablation and of electrical recording and stimulation. The other textbook had no chapters on any sensory system other than vision, and no separate chapter on sleep, arousal

and attention. In the present book there is a brief mention of evidence on humans in the section on the hippocampus, whereas in the earlier book there was none. The last difference is the inclusion in this book of summaries and reviews at the end of chapters or parts of chapters, these being added, according to the preface, to make the book more suitable for an introductory course.

It is necessary to assess not only what is included but also what is left out from a book of *Essentials of Physiological Psychology*. Some of the omissions are serious. The chapter on research methods contains no section on behavioural techniques, and no section on experimental methods in neuroanatomy. Yet a student could hardly be said to understand a behavioural deficit if he does not know how the behaviour is assessed, or to understand the neuroanatomy of the brain if he does not know how it can be shown that area A is connected to area B. The chapter on neuroanatomy is in the worst tradition of human neuroanatomy textbooks, concerned mainly with labelling, and only with the human brain. The secrets of neuroanatomy for students of physiological psychology are not contained in the cranial nerves. This chapter is also very sparsely and poorly illustrated, there being, for example, no figures for the hypothalamus or thalamus. One last respect in which the book omits essentials lies in the very scant coverage of neuropsychological data on humans. No mention is made of language or specialization of the hemispheres, in spite of the fact that the neuroanatomy chapter is all on man.

It might have been expected that the author would not only have selected the essentials from the previous book but also have brought it up to date. The chapters covering areas in which he has done research are up to date, those on hunger, thirst, emotion and neuropharmacology containing 25% of references dated 1967 or later. But the chapters on audition, vision, the anatomy of learning, and the reticular formation, none of them subjects in which he has worked, contain only 3.6% of such references. The discussion in these chapters hardly contains the essentials. No mention is made of recent work on stereoscopic vision, the discussion of frontal association cortex is at least 10 years out of date, and there is only a single paragraph on posterior association cortex, omitting everything of importance.

A textbook for an introductory course should not only contain the essentials but also interpret studies in a way which will help the student to grasp their significance. This is not done, for example, in the discussions of the results of single unit recording in visual cortex, or in the passing mention of studies in which

electrical stimulation of the same area of the hypothalamus in a rat may produce not only eating but also drinking or gnawing. Similarly possible differences between the effects of bilateral hippocampal lesions in man and other animals are mentioned, but no real attempt made to account for them. The reviews at the ends of chapters are of no help since they do little more than summarize. A student on an introductory course may find the chapters on motivation helpful, but for the rest he will not gain a grasp of the essentials of physiological psychology. There are other better and more interesting books which will help him to do that.

R. E. PASSINGHAM

Gases in Metals

Interaction of Metals and Gases. By J. D. Fast. Volume 2; Kinetics and Mechanisms. Pp. ix+318. (Macmillan: London and Basingstoke, January 1972.) £10.

In volume 1, published in 1965, Professor Fast covered the thermodynamics and phase relations of gases in metals. In volume 2 he deals with kinetics but excludes the topics of absorption, catalysis and oxidation on the grounds that these are adequately treated elsewhere. Volume 2 has only five chapters of which chapter 1, on solution of gases in metals, to some extent covers the material of volume 1 but with greater stress on the atomic interpretation of solution phenomena. The remaining chapters are on diffusion in interstitial alloys, internal friction in metals and the Snoek effect and interaction of interstitials with other lattice imperfections. The emphasis, therefore, is on the effect of foreign atoms on the structural and mechanical properties of metals.

By limiting his field, however, the author has again achieved a satisfying balance between theory and practice, as is appropriate to a professor who is also Chief Metallurgist to Philips Research Laboratories. Because the author intends the book for students of metallurgy as well as practising metallurgists, the theory of a particular phenomenon such as interstitial diffusion or relaxation is developed from elementary first principles, in both phenomenological and atomic terms and then applied to an abundance of experimental data on gas-metal systems. The many references to original work make this an invaluable source book for practising metallurgists. For students, who would benefit greatly from Professor Fast's clear expository style, I fear that the high price of the book for the restricted field covered may prove a deterrent. J. A. PRYDE

Marine Geochemistry

The Changing Chemistry of the Oceans. Edited by D. Dyrssen and D. Jagner. (Proceedings of the 20th Nobel Symposium held at Göteborg, August 1971.) Pp. 365. (Wiley: New York and London, October 1972.) £9.

THIS volume contains most of the papers and the informative discussions from a Nobel Symposium at which the main theme was the effects of man's activities on oceanic processes. These activities are increasing detectably the rate of movement of certain elements in natural cycles and introducing significant inventories of substances hitherto absent or at negligible concentrations. Increased knowledge is needed of the background levels and perturbations against which such changes may occur, and the processes governing the behaviour of the chemical forms concerned. It is appropriate then that the proceedings were concerned as much with the changing perspectives of our understanding of marine geochemical and biogeochemical processes as with pollution which can only be interpreted in the light of such information.

The balance of subject matter reflects the emphasis placed on atmospheric interactions and pathways, in current marine chemical research. Wind-blown dust as a component in pelagic sediments and an influence on trace metal distributions is discussed by Chester. The importance of aeolian pathways for pollutants is brought out by Goldberg in a synthesis with many quantitative insights into human influences on the sedimentary cycle. The transfer of particles from ocean to atmosphere is examined by Chesselet, Morelli and Menard, who also discuss the atmospheric gaseous chlorine, probably as hydrogen chloride, of marine origin. Hahn shows that parts of the Atlantic Ocean act as a source for atmospheric nitrous oxide also. The more fully explored carbon dioxide cycle is discussed here by Machta, with emphasis on the human perturbations of the inventory and seasonal atmospheric variations, and by Pytkowicz, who considers the long-term geochemical aspects. An important and rather neglected aspect of the air-sea boundary layer, namely the influence of surface films, is examined by Garrett.

Some information on changes in the chemistry of sea water on both recent and geological time-scales is recorded in the sedimentary deposits. Lal and Krishnaswamy describe investigations of ferromanganese concretions which, because of their dominantly authigenic character and slow kinetics of formation, may constitute especially valuable sedimentary records. Sedimentary pyrite formation and its quantitative

importance as a removal route for oceanic sulphate is discussed by Berner. Several papers describe the occurrence of specific pollutants and the behaviour of elements which can cause pollution problems in coastal waters. Some useful contributions give relevant information on physical, meteorological and biological processes. The importance of modelling as an aid to understanding highly complex environmental systems emerges as another pervasive theme, to which Odum contributes an outstanding and thought-provoking study of energy circuit modelling.

The papers were published only fourteen months after the symposium. Misprints and eccentricities of presentation, numerous in some parts, might have been removed at the cost of a longer gestation. Although the uninitiated may ponder on page 326 over the nature of the testes which are said to fall rapidly to the sea bed and dissolve, these errors do not seriously detract from the value of the publication. It deserves a wide readership.

J. D. BURTON

Irradiated Polymers

The Radiation Chemistry of Macromolecules. Edited by Malcolm Dole. Volume 1. Pp. xiv+369. (Academic Press: New York and London, November 1972.) \$23.

POLYMERS, like materials of other kinds, are prone to suffer damage when exposed to high energy radiations but it often is the case that the effects can be turned to good use. For example, cross-linking can be induced by suitable irradiation of a moulded plastic article in order to stabilize its shape and enhance its mechanical strength, a reaction distinguished from other post-treatments of comparable types in taking place throughout the specimen rather than in limited depth at the surface.

Great interest attaches to the radiation chemistry of polymers at the academic level as well as the technological; previous accounts of the subject are now a decade out of date and the volume under review is the first half of a two-part treatment which is obviously timely, if not overdue. To some extent this volume is preparatory in that it deals for the most part with the theoretical aspects of the subject, including the concepts of energy transfer in polymers, the nature of free radicals, molecular mobility and electrical conductivity.

There is also a chapter describing experimental techniques and the book ends with two discussions of radiation effects in relation to polyethylene. These latter contributions seem rather out of place after the very general foregoing matter, but they are really a pre-

lude to the second volume which will deal with a whole range of polymers, including crystalline, fibrous and biological materials.

The two final chapters really whet one's appetite for volume 2: they indicate the remarkable way in which the behaviour of high polymers can depend critically on minute details of structure and, conversely, on the way in which minimal changes in structure can produce drastic modification of properties.

While it seems unlikely that the earlier established works by Chapiro and Charlesby will become redundant, it is obvious that this book and its successor will be essential for any comprehensive library in a polymer science environment.

A. D. JENKINS

Essential Photosynthesis

Photosynthesis. By D. O. Hall and K. K. Rao. Pp. 68. (Edward Arnold: London, November 1972.) £1.50 cloth; 75p paper.

In this text (*Studies in Biology*, No. 37) intended for sixth-formers, their teachers and first-year undergraduates, Hall and Rao have set out a description of photosynthesis that is admirably clear and terse. After an introduction and history, they concentrate on sub-cellular aspects and explain light absorption (chapter 4) and electron transport (chapter 5) in chloroplast membranes (chapter 3) and the metabolic assimilation of CO_2 in their immediate environment (chapter 6). A comparative outline of bacterial photosynthesis follows (chapter 7).

This account, necessarily complex, is illustrated by a well-chosen series of electron micrographs and by clear diagrams explaining some crucial historical experiments, and others by which the student could corroborate the text. Recipes are given for chloroplast isolation and in chapter 9 some 15 practical experiments are listed (by reference).

The set length has restricted the discussion to the area which the authors can claim to be reasonably agreed. In chapter 8, however, there is a list of some of the regions where current research is most active. The suppression in the interests of brevity of details of, for example, the nature of redox carriers, or metabolic intermediates, also has some compensation in that each pathway can more readily be perceived as a whole by the student.

This approach must be successful provided that the text is used in situations where basic teaching can be relied on to establish the concepts of metabolic maps, electron transport, ATP and so on. In turn, the details that are provided will serve as an index for the student to enter the major literature and to tackle the many controversies yet to be resolved.

R. P. F. GREGORY

Frege on Geometry

On the Foundations of Geometry and Formal Theories of Arithmetic. By Gottlob Frege. Translated and with an introduction by Eike-Henner W. Kluge. Pp. xlii+163. (Yale University: New Haven and London, November 1971.) \$10; £4.75 cloth.

FREGE's articles on the foundations of geometry which are translated in this volume had their origin in a correspondence with David Hilbert about Hilbert's famous *Festschrift*. In the *Festschrift*, Hilbert constructed an axiomatic system for geometry and sought to prove the independence of the parallel axiom. Frege does not concern himself with the technical details of the system but with the fundamental questions whether the concepts of geometry can be defined intrinsically by means of axioms. In his reply to Frege's first letter Hilbert states his position in the following words: "If the arbitrarily posited axioms together with all their consequences do not contradict one another, then they are true and the things defined by these axioms exist." Frege contests this view in its entirety, arguing that there are no means of proving consistency other than by exhibiting an object with the properties demanded by the axioms, and if one has such an object what need is there to prove its existence by the roundabout method of consistency? Hilbert does not continue the correspondence, but the attack on Frege is taken up by Korselt who reiterates the standpoint of "modern mathematics". Frege's reply to Korselt is highly abusive: "I have been at pains," Frege says, "to draw sharp boundaries: Mr Korselt, it seems, diligently blurs them once again. How is this to be explained? Perhaps by a drive for self preservation on the part of Hilbert's doctrine for which an obscurity of the issue may well be a condition of survival." Again later in the same article he criticizes Korselt's changing conceptions of axioms, finding at least four different uses of the term. Frege rejects the axiomatic method for its failure to give references (denotations) to the words "point", "line", "plane" and so on, demanding of a definition (of a point for instance) that we will be able to judge by it of any object whether or not it be that which is defined. The kernel of Frege's opposition to formalism is that the propositions of mathematics are not mere sequences of words or groups of signs, but are thoughts, something which can be true or false.

In addition to the articles on the foundations of geometry the translator has included Frege's controversy with his Jena colleague J. Thomas about formal theories of arithmetic and contributes

a thirty page introduction which sets the scene and fills in the background to the controversies.

The subtlety of Frege's mind and his tremendous gifts for logical analysis and disputation are boldly displayed in the essays in this collection, but the judgment of history has gone against Frege and the axiomatic method in mathematics has prevailed against all the fury of his attack.

R. L. GOODSTEIN

Quantum Theory

The Quantum Theory of Atoms, Molecules and Photons. By John Avery. Pp. xv+378. (McGraw-Hill: Maidenhead, September 1972.) £8 cloth; £5.50 paper.

THE quantum theory of atoms, molecules and photons forms a central part of undergraduate and graduate courses in physics and chemistry. There are a number of very good textbooks covering this field, so it is reasonable to ask if a new one is needed. No doubt we could do without this book, but it will surely find some friends, particularly among physical and theoretical chemists.

The author is a graduate of the Massachusetts Institute of Technology and of the Universities of Chicago and London, and is a lecturer in the Chemistry Department at Imperial College, London. The book has been written for chemistry students who like mathematics. It uses advanced mathematical techniques when these are needed but it does not assume too much prior knowledge. The style is lucid and pleasing, although the first chapter, on the development of mechanics, is rather long.

There are nine chapters covering topics such as Hilbert space, angular momentum, the chemical bond, translational symmetry, vibration and rotation, and "ions and ligands". There are seven appendices of which the largest is a 23-page account of group theory. A few examples are given and some have hints for their solution. The book provides a modern approach to many topics of interest to chemists and physicists—phonon and photon creation and annihilation, the scattering of radiation, optical activity, and semi-empirical computations of molecular structure and properties. Some of these and other topics are only lightly touched upon, but the overall coverage will appeal to theoretically inclined research students in chemistry and to a very few undergraduates. The charm of the book is enhanced by amusing historical and biographical anecdotes. Very few mistakes were noticed, although Russell, who is often linked to Saunders, is misspelled throughout.

A. D. BUCKINGHAM

CORRESPONDENCE

Club of Rome

SIR,—The editorial in *Nature* of March 16 (242, 147; 1973) gives a brief review of the voluminous report of the Science Policy Research Unit of the University of Sussex on the *Limits of Growth* (*Futures*, March 6), and calls it, hopefully, "Almost the Last Word on the Club of Rome". This is liable to give a false impression of the Club of Rome, of the *Limits* and to some extent even of the Sussex Report.

In my opinion the Sussex Report is not so much an axe as, rather, so far the best constructive critique of the *Limits*. It is fair, courteous and thorough. Marie Jahoda in her "Postscript on Social Change" writes, "We know from our experience in studying Forrester's and Meadows's work that it took a group of highly trained experts in a variety of fields many months of hard effort to grasp fully the technique, the assumptions and the shortcomings of world dynamics". Almost every page of the Report bears this out. It is a very welcome change after the wholesale rejection of *World Dynamics* and the *Limits* by many economists, whom one must strongly suspect of not having read the books, but just leaped through them with disgust. The Sussex Group have read them, digested them and much of what they write, though not all of it, can be considered as a fair commentary, which will give the reader a deeper understanding of these works, while showing up some of their weaknesses.

The least satisfactory are the computer runs of the Sussex Group. They cast doubt on the reliability of the world models by running them backward, with the result that they "retrodict" a catastrophe in the past. I need not deal with this, as it has been answered by Meadows in the same number of *Futures*. An error of 1/1,000 in some parameters is liable to make the model run off its track in the past, while an error of the same order would be hardly noticeable in a forecast. The Forrester-Meadows models share this property with many other complicated physical systems. Nor need we be impressed with the difference which they find in forecasts by substituting expanding in place of shrinking resources. This is not a "small change" in the assumptions.

On the other hand, in view of what we have experienced from economists, it is an agreeable surprise to find that chapter 6, *The Capital and Industrial Output System* by Christopher Freeman with the economists Julien and Cooper,

is particularly constructive. They make it clear that it is hopeless to treat an aggregated system, in which Africans and Americans are lumped together, dynamically with a single set of operators. This is macroeconomics taken too far. One cannot expect from such a forcibly homogenized model anything but coarse approximations, and nothing more has ever been claimed by its authors.

This type of critique, of which there are many more examples in the long report, is welcome to the Club of Rome as a dialectical aid towards the next step. It does not find us at all unprepared. We have never considered the world models as oracles of unescapable fate. Perhaps the words "Predicament of Mankind" were misleading. A predicament is not a trap from which one can never escape; it was meant to be a warning and a challenge. A warning against "ostrichism" and a challenge to take up the fight with the dangers which have been revealed. As far as I can see, the Sussex Report nowhere states that the dangers are imaginary, though it often suggests that they might be exaggerated.

Are they exaggerated? I think that if the Club of Rome has anything with which to reproach itself, it is rather that it has not warned in time that while the global danger may be a hundred years ahead, its first serious wave is right upon us. We are far from living in a unified world in which the resources are equitably or at least reasonably shared. While the Club of Rome warned of an overshoot in the consumption of non-renewable resources some time in the next century, the highly industrialized nations and in particular the United States have long overshot the limit at which they could live safely within their own means. A grave fuel and energy crisis is now striking at the industrialized countries, long before the resources of the world are exhausted—because we have not foreseen it in time.

The fuel and energy crisis which is starting now is likely to last for two decades, and it is scant satisfaction for us of the Club of Rome that from now on we are likely to meet much less complacency, much less of the "I am all right, Jack!" attitude. When the Sussex Group states that it considers the political difficulties as more urgent and important than the physical limits, it is in perfect agreement with the Club of Rome. From the start its founders have considered international cooperation as their most important target, and they

were successful in establishing a worldwide network of intellects, not entirely without political influence.

There are many minor deficiencies in the Forrester-Meadows models which could be amended, but it is clear that no aggregated world model can be satisfactory. The Club of Rome has long understood this, and now, among other projects, it is sponsoring a *regionalized* computer model of the world. This ambitious project, headed by Professors Mesarovic and Pestel, divides the world into 10 regions, taking account of their self-interest and also of their political philosophy. If successful, this project may give answers to many questions which the *Limits* have left open. Only one thing is certain; the dangers, the instabilities, will be found very much nearer to us than the global physical limits.

As regards the ultimate, physical limits, we believe that these can be pushed further away, but we are not satisfied with pious hopes. The Club of Rome is going ahead with plans for mobilizing creative technology in all countries. We are not "doomwatchers" but doomfighters.

Yours faithfully,

DENNIS GABOR

Imperial College,
London

Princeton in Trouble

SIR,—Many of us at the Institute for Advanced Study were sad to read your leader about Princeton (*Nature*, 242, 217; 1973). Not only is your statement of the facts extremely misleading, but also your underlying philosophy is difficult to understand. Your writer seems to believe that an academic institution, by its very nature, should be a dictatorship, and that in case of a conflict between faculty and director the faculty must go.

The background of the present dispute can be described briefly as follows. It had been unanimously agreed that the faculty as a whole should supervise the formation of the fledgling "Program in Social Sciences" by examining the credentials of candidates for the first three professorships. Precisely because these first appointments would determine the future direction of the program, it had been agreed that such faculty supervision was essential; and in fact the program got off to an excellent start with the unanimous faculty approval of Clifford Geertz as the first professor.

Going further back, the following promise was made in 1947 by Frank Aydelotte, then director of the Institute: "I have given an undertaking to the faculty that I would never recommend to the trustees an appointment of which the faculty did not approve". This principle has been a firm tradition ever since, until the present altercation. It is our hope that negotiations now in progress between faculty and trustees will lead to a reaffirmation of Aydelotte's undertaking.

For readers who may have been misled by your headline, let me emphasize that the Institute has no connexion with Princeton University, which has a long record of excellent administration.

Your faithfully,

JOHN MILNOR

*The Institute for Advanced Study,
Princeton, New Jersey 08540*

Careers

SIR,—The worsening market situation for scientists and technologists described in your supplement, "Careers for Scientists", and the threat of further reductions in scientific establishments, especially in the largest companies, leads to one irresistible conclusion: the need for scientists and technologists to join this union set up by the Council of Science and Technology Institutes just over a year ago. It is one reason why membership is already 4,000 and climbing steadily.

Of course, as several of your contributors pointed out, the picture is not all black. More and more science graduates are entering managerial employment, which we believe to be essential if the science-based industries are to develop their full potential, or scientific work at a supporting level. Viewed, therefore, as a whole, we would agree with Mr Davies that we are not producing too many highly educated people and can do with more, provided that planning is sensible and that people are told what to expect. This, too, emphasizes how vital it is that scientists and technologists moving into unfamiliar fields make sure that they can avail themselves of the assistance which APST can provide.

Yours faithfully,

M. I. GILLIBRAND

*Association of Professional Scientists
and Technologists,
Hinchley House,
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London W1N 2BE*

Citation and Distinction

SIR,—Garfield¹ continues to make superficially plausible claims for the Science Citation Index, which his company publishes. Obviously Nobel Prize awards and citation frequency are partly

caused by the same factors. It may be coincidental that Nobel Prizes are not given in the evolutionary half of biology (with adjacent fields such as stratigraphic geology), where the literature is extraordinarily diverse and is poorly represented in the Index.

In Garfield's lists² of most-cited and highest-impact journals, eighty-one and seventy-one respectively of the first 100 are devoted exclusively to chemistry, physics, biochemistry, physiology, and medicine. This partly reflects the same bias in the source, but also the fact that there are, for example, more organic chemists than evolutionary biologists.

In a forthcoming work where I propose a new scientific law with considerable applicability, I make few references. But the result depends directly on, and would have been impossible without, a large proportion of all the work in evolutionary biology that has been done in the past. A simple-minded cost-effectiveness approach like Garfield's would have prevented this work from being done.

Any action taken on the basis of such biased results as Garfield's clearly discriminates against the areas under-represented. These areas are as a whole probably more dependent on extensive availability of literature than is any other area of science.

Yours faithfully,

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Illinois 60637*

¹ Garfield, E., *Nature*, **242**, 485 (1973).

² Garfield, E., *Science*, **178**, 471 (1972).

Human Papova Viruses

SIR,—We would like to correct some of the facts in your News and Views article "Human Papova Viruses" (*Nature*, **241**, 308; 1973).

A human papova virus of the polyoma virus subgroup "first came to light" when Zu Rhein and Chou observed by electron microscopy virus particles in brain cells of patients with progressive multifocal leucoencephalopathy (PML) (*Science*, **148**, 1477; 1965). Many attempts have been made to grow this virus *in vitro* without success.

Simultaneously with our report of the isolation of BK polyoma virus from the urine of a renal transplant patient (Gardner *et al.*, *Lancet*, **i**, 1253; 1971), Padgett and her colleagues described the successful culture of JC polyoma virus directly from the brain of a patient with PML (Padgett *et al.*, *Lancet*, **i**, 1257; 1971). It was unfortunate that no reference to this important work was made in your article. Later, Weiner and his co-workers reported the isolation of two further strains of polyoma virus from the brains of two patients with PML (*New*

Engl. J. Med., **286**, 385; 1972). Both of Weiner's isolates appear to be antigenically identical and biologically similar to the simian polyoma virus (SV40) whereas JC, the other virus isolated from PML, appears antigenically unrelated to SV40 by immunofluorescence.

Our BK polyoma virus has only a very minor antigenic cross-reaction with SV40 and shows many differences from this virus biologically, including the property to agglutinate human O and guinea-pig erythrocytes to high titres. It seems therefore that we are indeed dealing with a group of different human polyoma viruses. In the discussion of human papova viruses it must not be forgotten that human common wart virus is also a member of the papova virus group although it belongs to the papilloma virus subgroup.

Yours faithfully,

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Announcements

Miscellaneous

Elections to the US National Academy of Sciences:

Edward Hamblin Ahrens, jun., **Rockefeller University**; Robert Wayne Allard, **University of California, Davis**; Andrew Alm Benson, **Scripps Institution of Oceanography**; Howard Alan Bern, **University of California, Berkeley**; James Daniel Bjorken, **Stanford University**; Harold Charles Bold, **University of Texas**; John Tyler Bonner, **Princeton University**; Frederick Herbert Bormann, **Yale University**; Gordon Howard Bower, **Stanford University**; Felix Earl Browder, **University of Chicago**; Donald David Brown, **Carnegie Institution of Washington**; Arthur Earl Bryson, **Stanford University**; Bernard Budiansky, **Harvard University**; John Werner Cahn, **Massachusetts Institute of Technology**; Robert Merritt Chanock, **George Washington University**; Albert McCavour Clogston, **Sandia Corporation**; Ansley Johnson Coale, **Office of Population Research, Princeton, New Jersey**; George C. Cotzias, **Brookhaven National Laboratory**; Philip Ernest Converse, **University of Michigan**; Ellis Brevier Cowling, **North Carolina State University**; James Edwin Darnell, jun., **Columbia University**; Albert Dorfman, **University of Chicago, School of Medicine**; Otis Dudley Duncan, **University of Michigan**; Isidore Samuel Edleman, **University of California Medical Center, San Francisco**; Walles Thomas Edmondson, **University of Washington**; Edmond

Henry Fischer, University of Washington; Marshall Fixman, Yale University; Robert William Fogel, University of Chicago; Robert Elder Forster, University of Pennsylvania Graduate School of Medicine; Donald Sharp Fredrickson, National Heart and Lung Institute; Gerhart Friedlander, Brookhaven National Laboratory; Milton Friedman, University of Chicago; Theodore Henry Geballe, Stanford University; Clifford James Geertz, Institute for Advanced Study, Princeton; James Freeman Gilbert, University of California, San Diego; Harry Goldblatt, Case Western Reserve University; Ludwik Gross, Mount Sinai School of Medicine; Gordon G. Hammes, Cornell University; Zellig Sabbatai Harris, University of Pennsylvania; Marion Frederick Hawthorne, University of California, Los Angeles; David Mark Hegsted, Harvard School of Public Health; Robert Fleming Heizer, University of California, Berkeley; Richard Marx Held, Massachusetts Institute of Technology; John Joseph Hopfeld, Princeton University; Kurt Julius Isselbacher, Harvard Medical School; Percy Lavon Julian, Julian Associates Inc.; Joseph Jacob Katz, Argonne National Laboratory; Joseph Bishop Keller, New York University; Lawrence Robert Klein, University of Pennsylvania; William Lester Kraushaar, University of Wisconsin; Edwin Gerhard Krebs, University of California, Davis; Aaron Bunsen Lerner, Yale University School of Medicine; Choh Hao Li, University of California, Berkeley; James Ross MacDonald, University of Texas; James Gardner March, Stanford University; Paul A. Marks, Columbia University; Jacob Marschak, University of California, Los Angeles; Victor Almon McKusick, Johns Hopkins University School of Medicine; William Burdette McLean, Naval Undersea and Development Research Center, San Diego; Stanley Lloyd Miller, University of California, San Diego; Raymond David Mindlin, Columbia University; Marvin Lee Minsky, Massachusetts Institute of Technology; Beatrice Mintz, University of Pennsylvania; Franco Modigliani, Massachusetts Institute of Technology; Yoichiro Nambu, University of Chicago; Gerry Neugebauer, California Institute of Technology; Bernard More Oliver, Hewlett-Packard Company; Alwin Max Pappenheimer, Harvard University; Robert Ghormley Parr, Johns Hopkins University; Rowland Pettit, University of Texas; David Pines, University of Illinois; John Michael Prausnitz, University of California, Berkeley; Charles Henry Rammelkamp, Case Western Reserve University; Simon Ramo, Bunker-Ramo Corporation; Helen M. Ramney, State University of New York at Buffalo; Lester James Reed, University of Texas; William Lawson Russell, Oak Ridge National Observatory; Jarvis Edwin Seegmiller, University of California, San Diego; Irving Ezra Segal,

Massachusetts Institute of Technology; Richard Burton Setlow, Oak Ridge National Laboratory; Robert Phillip Sharp, California Institute of Technology; Horton Guyford Stever, National Science Foundation; Julina Munson Sturtevant, Yale University; Helen Brooke Taussig, Johns Hopkins Hospital; Kip Stephen Thorne, California Institute of Technology; William Trager, Rockefeller University; Merton Franklin Utter, Case Western Reserve University; Cecil Herbert Wadleigh, US Department of Agriculture; Robert Mowbray Walker, Washington University; Anthony Francis Clarke Wallace, University of Pennsylvania; Donald Edward White, US Geological Survey; Gilbert Fowler White, University of Colorado; Maxwell Myer Wintrobe, University of Utah School of Medicine.

Reports and Publications

not included in the Monthly Books Supplement

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Progress in Reaction Kinetics, Vol. 7, Part 1. Edited by K. R. Jennings and R. B. Cundall. Pp. 82. (Oxford and New York: Pergamon Press, 1973.) £1.75. [72]

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- US Department of the Interior: Geological Survey. Paper 1663-H: Origin of Mineralized Water in Precambrian Rocks of the Upper Paraíba Basin, Paraíba, Brazil. By Stuart L. Schoff. Pp. iv+38+plate. 1. 60 cents. Paper 1873-E: Movement and Dispersion of Soluble Pollutants in the Cape Fear Estuary, North Carolina. By E. F. Hubbard and William G. Stamp. Pp. v+31. 25 cents. Water-Supply Paper 2008: Sediment Transport on a Mississippi River Distributary—Bayou Lafourche, Louisiana. By W. Harry Doyle, Jr. Pp. v+48. 40 cents. Professional Paper 729-C: Stratigraphic Framework of the Absaroka Volcanic Supergroup in the Yellowstone National Park Region. By Harry W. Smedes and Harold J. Prostka. Pp. 33. 50 cents. Professional Paper 739: Geology of Isla Deseccho, Puerto Rico, with Notes on the Great Southern Puerto Rico Fault Zone and Quaternary Stillstands of the Sea. Pp. v+22. 45 cents. (Washington, DC: Government Printing Office, 1972.) [62]
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